THE QUARTERLY

JOURNAL OF SCIENCE.

OCTOBER, 1876.

I. JAPANESE MINES.

It is certain that in almost every portion of the empire of Japan minerals of various kinds are found, and it is equally certain that in numberless parts of the country mines have been worked, with more or less success, for centuries. So says the Hon. F. R. Plunkett in a recent Report to our Foreign Office, and under his guidance we will endeavour to see how, unaided by the means and appliances of modern science, that strange and long-isolated race—with whose ways we are only now, in the last quarter of the nineteenth century, gradually becoming somewhat more familiar—contrived, during her ages of seclusion, to make the earth give up her mineral wealth. The interior of Japan and its resources are still so little known to foreigners that Mr. Plunkett, even with the advantages of his official position, could hardly have hoped to achieve any very satisfactory result in the course of his investigations, had not the Japanese Government placed at his disposal the valuable information in their possession, and had not their chief mining engineer, as well as the metallurgist of the Imperial Mint at Osaka, rendered him most efficient aid in the prosecution of his researches. With such assistance, however, and by careful observation on his own part, he has collected a mass of useful statistics, as well as much curious and interesting information.

With one solitary exception, where foreign skill has been availed of with good results, the mines of Japan are still attacked exclusively by adits, for the natives never sink a shaft, and, as they have nothing more powerful than bamboo pumps for lifting water, it is easily conceivable how soon it gets the upper hand, and how the number of abandoned mines comes to be so large.

Coal-mines occupy the most important place among the mining industries of Japan, and the best developed and most productive of these is undoubtedly in the small island of Takashima, some 10 miles from Nagasaki. Up to 1868 this mine produced but a small quantity of coal, owing to the defective system of mining pursued by the natives, viz., working the seam from its highest level, where the coal strata crop up to the surface, and following it downwards in the direction of its dip. Since that time, however, the mine has been worked under foreign superintendence, the result being a vast improvement in the amount and quality of the output. The district of Karatsu, to the north of Nagasaki, contains several coal-mines, little inferior to the foregoing, which are still worked in the native manner, and of one of which we will give a brief description. The road to these mines runs inland to a distance of nearly 9 miles from the town, passing up a valley of singular beauty and richness, from 1 to 3 miles wide, hemmed in by mountains, most of which are clothed with luxuriant forests to their very summits: the lowland is all well cultivated, and produces rice and cotton; while along the foot of the mountains wheat, millet, beans, and the paper-tree grow in great quantities. The river which runs through this valley, nearly parallel to the road, is a regular mountain stream, with a broad bed and little depth of water, but it is just sufficient to float the small flat-bottomed boats carrying the coal down to the sea. The mine, visited by Mr. Plunkett, has but one entrance, viz., an adit bored through the rock, about 4 feet wide and 3½ feet high, carried down at an angle of 56° until it reaches the seam of coal about 100 yards from the entrance. The mode of working the coal and getting it up to the surface is very peculiar. The miners work lying on their sides or backs, and never can stand up. The coal is removed with small picks and wedges driven in with a hammer; it is then placed on small oblong bamboo baskets, each holding rather more than 1 cwt., provided with iron runners, which just fit the wooden sides of the ladder, laid along the floor of the entrance adit. This forms a kind of rude tramway, by which the baskets are hauled up to the surface. They are drawn by boys, apparently not more than 12 or 14 years old, who crawl on all-fours, harnessed to the basket by a very short yoke,—so short, indeed, that even when out of the mine, and dragging their loads across to the coal-yard, they are still obliged to crouch in the same painful manner.

They haul their loads up the incline, holding with their hands and feet to the ladder before mentioned. The mine is lighted with common open oil-lamps, and no precautions are taken against fire-damp (which, owing to surface coal only being worked, does not seem to be troublesome in Japanese mines); its ventilation below is described as atrocious, and the means for getting rid of water are primitive in the extreme, so much so that Mr. Plunkett's account of them deserves to be quoted. The working galleries being only about 70 feet below the surface, just above these a large square well is dug to a depth of about 50 feet, and the water from below is pumped into this by small hand bamboo pumps, capable of lifting about as much water as an ordinary garden syringe. On the surface, close to one side of this well, stands a high post, on which works a long walking beam, at one end of which, fastened to a thick bamboo, is a large tub; the bamboo is long enough to allow the tub to reach the bottom of the well; at the other end of the walking beam are attached four ropes. Inside the well, about 10 feet down, is a small platform, on which a man stands to receive the tub as it comes up. Suppose the tub to be at the bottom of the well, four men, holding the ropes at the end of the walking beam, run down the side of a small mound, thereby lifting the tub to the level of the platform, where the man, seizing it, turns the water over into a small channel made to carry it out into the valley, which is a few feet below, and where there is a small stream communicating with the river further down. The four men then walk up the hill again, thereby letting the tub fall to the bottom of the well to be refilled, and so on till the water is reduced to a proper level. The coal is taken from the pit's mouth to the river-side in small carts, which merit a few words in passing. They are of two kinds; one being a bamboo basket placed on low wheels, and fitted at each end with semicircular shafts bending downwards, so that the hind shafts can be made to scrape along the ground, and thus act as a drag; and the other a narrow wooden box, mounted on two high wheels, and fitted with a pair of shafts about 7 feet long, joined together at the end. Both these conveyances carry from 2 to 3 cwts. of coal, and are equally clumsy; the only difference between them is that, whereas the baskets are dragged, the boxes are pushed, and, as a rule, men are employed to work the latter and women the former.

Copper, which ranks next in importance among the minerals of Japan, is found in numberless parts of the country, and is usually of excellent quality. When properly refined it takes a foremost place amongst the various kinds of commercial copper, and it is almost invariably free from the injurious metals, such as antimony and arsenic. A brief account of a copper-mine in Yamato, in the neighbourhood of Osaka, will probably possess some interest, inasmuch as from various circumstances it is doubtful, in the opinion of the metallurgist of the Japanese Mint, whether European processes would profitably and successfully replace the native methods at present followed. This mine is situate in a narrow valley in the midst of mountains, and to reach it two passes, nearly 3000 feet high, have to be crossed. So narrow, indeed, is this valley, and so steep the inclination of its sides, that there is no level ground, nor is there ground which could be levelled, within nearly half a mile of the entrance of the mine, and all the present dressing-houses, furnaces, &c., are perched upon the slopes, and supported by built-up foundations. The levels in this mine are driven and the ore removed by the aid of gunpowder, and the pieces broken down are carried to the surface on the backs of men and women. The ore, having been broken up into pieces about the size of walnuts, is placed, with a sufficiency of wood, in kilns 5 feet square, with walls rudely built of stone and lined with clay. The calcination lasts fifteen days, but is of a very unsatisfactory character, much of the ore undergoing but slight change in the process. The furnace consists of a hemispherical cavity in the ground, lined with refractory clay: it is furnished with two bellows at the back and one in front, and is half covered with a thick semicircular lid of clay. The process of melting is thus described by our authority:—The furnace, having been repaired and well dried after the "heat" of the previous day, is lighted by introducing some burning embers, together with fresh charcoal, and over this charcoal a third part of the charge of ore is piled, forming a heap; the blast from the bellows behind is started, and soon jets of flame of carbonic oxide make their appearance over the whole surface of the heap, which, gradually melting, subsides into the furnace; the slag is then removed, and another portion of charcoal and of ore piled up as before. When the whole of the charge (about 11 cwts.) has been melted down in this way, the slag is skimmed off; after which the front bellows is brought into action, the blast of air being directed upon the surface of the molten "regulus" and copper, and a little charcoal added at the side from time to time to keep up the heat. In ten hours the charge is worked off, and the copper lifted out, in more or less circular cakes, by throwing water on its surface and raising the solidified crust with an iron tool. This process has many advantages for a country like Japan, where the means of intercommunication are so bad, and coal is, therefore, not available as fuel.

Japan possesses both gold and silver, although not in very large quantities. Mr. Plunkett gives no account of these mines in his Report; in fact he does not believe that they are at present of much value. As the Japanese Government has recently turned its attention to their development, and as considerable interest attaches to the subject, we will condense into as brief a space as possible—from the columns of the "Hiogo News," an English newspaper published in Japan—an account of the silver mines of Ikouno, in Tajima. On the hill-side, above the works, are to be seen a number of places like large rabbit-holes, and a tramway which runs round the face of the hill, connecting the holes with a series of shoots, by means of which the ore is passed down to the works. Entering one of the holes referred to, which prove to be the mouths of galleries, we see the first process of removing the ore by blasting, the fuses for which are now all made on the premises. Emerging, we find the ore at the deliveries of the shoots being broken with hammers into pieces, varying in sizes, the richest portions being broken the smallest. The poorer lumps, and those which contain a preponderance of other minerals, are set aside for consumption at convenience. The more choice morsels are set out, according to quality, in five classes of an estimated value, on appearance of bearing silver in the proportion of from 80 dollars per ton to 5000 and upwards. These fragments are then pounded into dust in crushing-mills, and the dust baked in ovens with common salt. Hitherto the silver has been combined with sulphur, but in these ovens a chemical change takes place, the chlorine of the common salt combining with the silver (and the 10 to 12 per cent of gold which the ore contains), and the sulphur combining with the sodium of the salt to make Glauber's salt, which is sent into the river. The ore—now a red earth—is next, by means of water and iron balls, thoroughly mixed with a large quantity of quicksilver, by the aid of revolving drums. Under this process the quicksilver takes up the precious metal, and when the amalgamation is complete the drums are emptied, and the now comparatively valueless mud washed away. The combined, and still fluid, metals are then treated with hydraulic pressure against a leather sieve, through which free mercury is extruded, leaving a putty-like, brilliant, white amalgam. Heated in iron retorts, the remaining mercury contained in this amalgam is driven off into a condenser, to be used over and over again. The resulting lumps of metal having been fused with borax, which brings away some scoriæ and other impurities in the form of scum, are run into moulds, and sent to the Mint at Osaka. The metal contains in this form, in which it leaves Ikouno, about 70 per cent of pure silver and 10 of gold, the remaining 20 per cent being nearly pure copper. When M. Coignet, the French superintendent of the Ikouno mines, was first appointed, the Government for a long time hesitated to admit that, even with the inferior appliances at his command, he could produce 4000 dols. a month of silver, but being once convinced they expended 400,000 dols. on European machinery, which arrived some three years ago, and, by the aid of a foundry and machine shop on the premises, supplemented by the efforts of the Yokoska Arsenal and the Kobe Iron Works, was soon got into working order. By last accounts the out-turn averaged 30,000 dols. a month, half of which was required for working expenses, leaving the Government a very handsome profit. We are informed that the power utilised to drive the machinery is mainly water, brought 4½ miles in an artificial canal, which is available for nine months out of twelve: during the rest of the year the works are driven by steam, for which purpose there are four engines of 25 horse-power each. In addition to the silver, which is at present the principal object sought, the hills contain copper, iron, lead, and zinc, the first of which it is in contemplation to work independently.

Iron is found in many parts of the country, and the total production thereof ranks next to coal in quantity, though not in value. The following is a brief account, given by Dr. Geerts, of Nagasaki, of the mode adopted by the natives for smelting the iron ore:—After the ore has been selected it is piled up in heaps with coal, and calcined (roasted) in order to expel the water, carbonic acid, sulphur, &c. This calcination makes the ore more porous, and better fitted for the smelting process. The calcined ore is smelted in a cylindrical furnace, built up with a few hard stones and fire-proof clay. The clay is laid in layers till the wall of the furnace has sufficient thickness. The thick bottom of this small furnace has a rounded shape, and a little above the bottom two exactly opposite openings in the wall are made for receiving the tubes of the bellows. Besides, there is a third opening near the bottom, which is closed with a clay stopper, and afterwards is opened to collect the fluid metal in the "forms." Now the furnace, previously perfectly dried, is filled with a mixture of coarse calcined powdered ore, charcoal, and some felspar, clay, or another quartz containing stone. The latter substances are added to act as a flux, and to separate the metallic iron from the impurities which are taken by the slag: sometimes, but not generally, coal or coke is used as fuel. When the heat produced by the continuous strong stream of air, pressed into the furnace by means of large Chinese bellows worked by four or five men, has been sufficient to smelt the ore, the iron will gradually run in a liquid state to the bottom of the furnace, and is cast in sand forms by removing the clay stopper of the lowest opening. The cold metal is sometimes purified by a second smelting, in another similar but smaller furnace.

Lead is found in many of the provinces, but, owing to the defective way in which the mines are worked, the annual production is but small. The following is, in brief, a description of a lead-mine near the famous Lake Biwa,—at no great distance from Kioto, the ancient capital of the Mikados,—and of the mode of smelting still practised there. The mine is worked by levels driven into the side of the hill, and the water is got rid of by an adit. The levels are driven entirely at random, and though there are native plans of the mine, they give no accurate information, either as to the direction or extent of the workings. Wood is used for fuel in the calcining kilns, and charcoal in the smelting furnaces. The charcoal is that of oak, maple, &c., for although the cryptomeria is the most abundant tree, charcoal made from it is disliked, because it is soft and swift-burning. The calcining kilns measure 4 feet every way, and are open at the top; their sides are of clay, and usually four, six, or even more, are built side by side in a row. At the bottom of the front of each there is a hole, which is opened or closed when necessary, according to the rate of combustion. The ore, having been previously broken up into small pieces, is placed in these in alternate layers with wood, and undergoes a first calcination for five days: the process is afterwards repeated in other kilns. The smelting furnace is situated under a rude chimney or hood of wicker-work, plastered with clay, open in the front to about the height of 6 feet from the ground, while its back wall screens the men who work the bellows from the heat of the furnace. The furnace is almost the same as that spoken of in connection with copper-smelting. Below the lid, at the back of the furnace, are two holes, by which the blast is admitted, it being conveyed thither by clay tubes running just below the surface of the ground. The daily charge of a furnace is 600 lbs. of calcined ore, divided into four equal parts; one part being charged into the furnace every hour and a half, and the whole worked off in six hours. The furnace having been lighted by introducing a small quantity of ignited charcoal, a quantity of charcoal—a little less in volume than the 150 lbs. of ore—is filled in; on the top of this the calcined ore is spread, covering it entirely and uniformly, and the heap is then gently patted with a small flat iron tool, and the bellows started gently. The remainder of the process is so much like that already spoken of in the case of copper smelting that we need pursue the description no further.

Besides the mineral productions already alluded to, tin, quicksilver, sulphur, and coal-oil are found in various parts of the Empire of the Rising Sun, but the mines, &c., require development by foreign aid before they can attain any considerable importance.

In conclusion, we may remark that Mr. Plunkett is not very hopeful as to the future prospects of mining in Japan, and he thinks it extremely doubtful whether there are many mines of such position, percentage, and character as would justify the investment of much capital in mining enterprise at present: he, further, expresses a belief that the mineral wealth of Japan has been hitherto estimated by the public far beyond its real value. In a recent article on the subject, the journal which has supplied us with particulars respecting the Ikouno mines ventures to entertain a more sanguine view of the matter, and states that "the more familiar acquaintance with the national resources, which we are always acquiring, satisfies us that on her store of minerals Japan must depend for her future prosperity." "It may be," the writer adds, "that we shall live to see a large export mineral business done, notwithstanding the obstruction which local and personal interest offers to its development; but whether we do or not, we shall not cease to believe that Japan will have to depend upon her mineral wealth for the place in the future she will take among other nations."

II. THE CRADLE OF CIVILISATION.*

THINKERS of no mean standing† have insisted strongly on the fact, real or supposed, that no nation claims civilisation as an indigenous product, but that all speak of it as having been originally introduced from abroad. Hence they infer that its source must have been a revelation communicated to man by some Divine, or at least superhuman, agent. With all deference to such high authorities, we submit that the premisses, even if indisputable, do not by any means justify the conclusion. Were civilisation the result of supernatural instruction, this must still have been imparted to men in some country or countries. Those men would then hold, as regards their neighbours, the very same position as if the lessons they had to communicate were the fruits of their own inventive genius, and the country where the supposed revelation had taken place would be regarded by the rest of the world as the fountain-head of civilisation, and would doubtless claim for itself this proud distinction. According to the supernatural, as well as to the natural theory of the origin of culture, it must be indigenous in some one country, if not in more, and the traditions which everywhere ascribe it an extraneous source can be regarded merely as a proof of the unreliability of early history. Indeed these old legends are in their nature subjective rather than objective, and have their roots in a primitive tendency of the human mind which might be named "elsewhereism" or "alibism." Barbarians, young persons, and the imperfectly educated invariably conceive everything great and wonderful as coming from afar. In their own country, wherever that may be, all is tame, mean, and common-place. But in the dim distance, "where the rainbow touches the ground," lie hidden treasures. In some remote isle flows the fountain of eternal youth. Across the seas, beyond the mountains, at the head-waters, or at the mouth of the great rivers, wealth, fame, wisdom, might be easily won. This feeling is often the cause of emigration. Men believe that they may easily find "elsewhere" the success which has escaped them at home. If their new home proves no more propitious than the old one, they merely conclude that it is not the right "elsewhere." Had they but gone to Canada instead of to South Africa all would have been well with them.

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* The Oera Linda Book, from a Manuscript of the Thirteenth Century. The original Frisian text, accompanied by an English version of Dr. OTTEMA'S Dutch translation, by W. I. SANDBACH. London: Trübner and Co.

† As, for instance, Archbishop Whately.

As with wealth, so with knowledge: "he has travelled for his wisdom" is still a common expression. We have heard the discoveries of such men as Humboldt, Agassiz, Darwin, and Wallace, ascribed not to their genius or their industry, but simply to the fact that they had visited countries remote. Any man who had been where they went might, it is asserted, have done as much. If such notions are still current in our days, need we wonder that in more primitive times all valuable truths were supposed to have been imported from abroad? A philosopher had travelled, and on his return imparted some valuable lesson to his countrymen; forthwith the *post hoc* was converted into the *propter hoc*, and the story ran that he had learnt all this elsewhere; yet the very legends betray themselves. Mr. G. H. Lewes, if we remember rightly, points out how a Greek sage is represented as having studied mathematics under the Egyptian priests, and yet at the same time as having taught them how to measure the height of the pyramids by their shadows! Surely they who required such an elementary lesson would have nothing very valuable to communicate. Nay, even if an originator had not travelled, his countrymen, rather than give honour to a prophet who had sprung up in their midst, would invent some legend of distant travel. This view, which no close observer of human nature can dispute, throws a flood of light upon all the Manco Capacs and other mysterious strangers who suddenly appear among savage tribes, and who, instead of being eaten, are at once accepted as high-priests, legislators, and kings.

We do not, of course, mean to deny that primitive nations learnt much from abroad, and that travellers from time to time brought back valuable lessons. When the means of communication between countries were scanty, philosophers had to travel in person for knowledge, just like merchants for commodities. We seek merely to show the true origin of the misleading sagas which ascribe all improvement to an unknown source, which, like the mirage, vanishes as we pursue.

However, if we may believe certain enthusiastic partisans, the true cradle of our modern civilisation has been at last discovered. The favoured spot is certainly not one which would be guessed by any unprepared reader. It is none of the regions which the researches of historians, antiquarians, and philologists have indicated as probable. It is not in Egypt or among the "blameless" Ethiopians; not in Assyria or Medea, India or China. We must not seek it in the ruins of Central America, or in that yet more mysterious "City of the Morning Star" whose palaces and temples extend for many a league amidst the forests of Siam. It is in that Dutch province known as Friesland!

It appears that a certain C. over de Linden, Chief Superintendent of the Royal Dutch Dockyard at the Helder, is in possession of a very ancient manuscript, which has been preserved in his family from time immemorial, its origin and contents being totally unknown, though a tradition had been handed down requesting its careful preservation. The manuscript is said to have been left to C. over de Linden by his grandfather, De Heer Andries over de Linden, who formerly lived at Enkhuizen, and died there, in the year 1820, at the age of 61. As the present proprietor was then a child, the precious document was preserved by his aunt, Aafje Meylhoff, born Over de Linden, and living at Enkhuizen, who, in the year 1848, considered that he had reached a sufficiently discreet age to take the family treasure into his own custody.

A rumour concerning the manuscript reached a certain Dr. E. Verwijs, who received permission to examine and copy it, and "was of opinion that it might be of great importance, provided that it was not suppositious;"—a very judicious reservation. However, his copy soon after fell into the hands of a Dr. Ottema, a less critical or a more excitable inquirer, who very soon succeeded in convincing himself of the great age and of the authenticity of the document. His method of reasoning may be gathered from the following paragraph:—

"In old writings the ink is very black or brown; but while there has been more writing since the thirteenth century, the colour of the ink is often grey or yellowish, and sometimes quite pale, showing that it contains iron. All this affords convincing proof that the manuscript before us belongs to the middle of the thirteenth century, written with clear black letters between fine lines carefully traced with lead. The colour of the ink shows decidedly that it does not contain iron. By these evidences the date given, 1256, is satisfactorily proved, and it is impossible to assign any later date. Therefore all suspicion of modern deception vanishes."

Dr. Ottema would perhaps be surprised to learn that these evidences amount absolutely to nothing. He does not appear to have subjected the writing to any chemical test so as to prove the absence of iron. He assumes the writing to be very old, and then infers, from its deep blackness, the absence of iron! But even supposing Dr. Ottema's conjecture to be correct, and that the ink is free from iron, this proves nothing. "Fine lines carefully traced with lead" can be produced in this degenerate nineteenth century quite as well as in the thirteenth. As a contemporary remarks, "A modern scribe is under no legal or moral obligation to put iron in his ink,"—more especially if his object is the not very moral one of literary forgery. There is therefore nothing in the ink or the characters which can speak in favour of 1256 rather than 1847 as the date of its composition.

The work professes to give fragments of the early history of the Frisians from the date of 2193 B.C. down to the times of Alexander the Great and his immediate successors. The author of the latter part of the book is assumed to have been a contemporary of Julius Cæsar, whilst the first portion, written by Adela, extends backwards to 558 B.C. Here the critical—not to say the sceptical—reader will be at once struck with a new difficulty. Admitting the manuscript to be authentic, and granting that its successive portions were first committed to writing at the dates just mentioned, what bridges over the gulf of 1500 years between the composition of the book and the earliest events therein recorded? If books, who preserved them, and who guarantees their authenticity or their very existence? If tradition, in how far can it be accepted? Suppose, for instance, that the only existing accounts of Julius Cæsar were found in a work ostensibly written A.D. 1500, would they not be looked upon with great and deserved suspicion?

The alleged Frisian chronology takes its rise from the submersion of the old land, Aldland or Atland, which extended "far to the west of Jutland, and of which Heligoland and the islands of North Friesland are the last barren remnants." This event, we are told, "is known by geologists as the Cimbrian flood." Now, that a great submergence of land may at one time or other have taken place where now the North Sea extends is not to be denied. It is supposed, with good show of reason, that Britain was once part and parcel of the European continent, but has been severed either by an extensive subsidence or by an alteration in the position of the earth's centre of gravity which deepened the waters in northern latitudes. But Dr. Ottema, on the faith of the "Oera Linda Book," seeks to identify this vanished Atland with Plato's Atlantis. It is true all earlier tradition has placed the site of this region in the Atlantic to the West of Africa, and a modern geologist has even—on very strong grounds—suggested that it formed the lowlands of a continent of which the West Indies are the more mountainous parts now alone remaining above the sea-level. Nothing, however, will satisfy Dr. Ottema but that Atlantis was a north-western prolongation of Holland. To one important point he makes no reference. Greek tradition represents the inhabitants of Atlantis as organising a vast expedition for the subjugation of the countries bordering on the Mediterranean, but as being repulsed by the Athenians. In the "Oera Linda Book" the Frieslanders are always represented as coming on friendly errands. The story of the disappearance of Aldland, Atland, or, if the reader will, Atlantis, is highly interesting, and may be made to serve as a crucial test for the authenticity of the entire book. We shall therefore quote in full :—

"Before the bad time came our country was the most beautiful in the world. The sun rose higher, and there was seldom frost. The trees and shrubs produced various fruits which are now lost. In the fields we had not only barley, oats, and rye, but wheat which shone like gold, and which could be baked in the sun's rays. The years were not counted, for one was as happy as another.

"On one side we were bounded by Wr-alda's Sea, on which no one but us might or could sail; on the other side we were hedged in by the broad Twiskland (Germany), through which the Finda people dared not come, on account of the thick forests and the wild beasts.

"Eastwards our boundary went to the extremity of the East Sea,* and westward to the Mediterranean Sea; so that, besides the small rivers, we had twelve large rivers given us by Wr-alda, to keep our land moist and show our seafaring men the way to the sea. The banks of these rivers were at one time entirely inhabited by our people, as well as the banks of the Rhine from one end to another. Opposite Denmark and Jutland we had colonies, and a Burgtmaagd (probably in Norway and Sweden). Thence we obtained copper and iron, as well as tar and pitch, and some other necessaries. Opposite to us we had Britain, formerly Westland, with her tin-mines.

"Britain was the land of the exiles, who, with the help of their Burgtmaagd, had gone away to save their lives; but in order that they might not come back they were tattooed with a B on the forehead, the banished with a red dye, and the other criminals with a blue. Moreover, our sailors and merchants had many factories among the Krekalanders and in Lybia. As our country was so great and extensive we had many different names.

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* Doubtless the Baltic, still called Ost-see—East Sea—by the Germans.

"Those who were settled in the higher marches bounded by Twisklanden (Germany) were called Saxmannen, because they were always armed against the wild beasts and the savage Britons."

How the bad time came:—

"During the whole summer the sun had been hid behind the clouds, as if unwilling to look upon the earth. There was perpetual calm, and the damp mist hung like a wet sail over the houses and marshes. The air was heavy and oppressive, and in men's hearts was neither joy nor cheerfulness. In the midst of this stillness the earth began to tremble as if she was dying. The mountains opened to vomit forth fire and flames. Some sank into the bosom of the earth, and in other places mountains rose out of the plains. Aldland, called by the seafaring men Atland, disappeared, and the wild waves rose so high over hill and dale that everything was buried in the sea. Many people were swallowed by the earth, and others who had escaped the fire perished in the water.

"It was not only in Finda's land that the earth vomited fire, but also in Twiskland. Whole forests were burned one after the other, and when the wind blew from that quarter our land was covered with ashes. Rivers changed their course, and at their mouth new islands were formed of sand and drift.

"During three years this continued, but at length it ceased, and forests became visible. Many countries were submerged, and in other places land rose above the sea, and the wood was destroyed through the half of Twiskland. Troops of Finda's people came and settled in the vacant places. Our dispersed people were exterminated or made slaves."

The question now arises whether this catastrophe, which must more or less have been felt throughout Europe, can be traced and identified? The phenomena described are of a twofold nature: there is, on the one hand, a permanent deterioration of climate, the sun no longer attaining its former altitude, frosts—formerly rare—becoming common, and wheat ceasing to ripen. The first-mentioned change, if it took place at all, would in all probability have more or less affected the whole world, and would doubtless have been recorded in the traditions of other nations. But how could such a change be effected? An alteration in the plane of the ecliptic could produce no such result if we take the entire year into consideration, since if the sun attained a less altitude at the one solstice it would be greater at the other. Nor could a change in the eccentricity of the earth's orbit. The only occurrence that could cause the sun to attain a lower general altitude at any place than had been formerly the case would be the removal of such place farther from the Equator, and consequently nearer to the Pole. Now, such changes have indeed been known to occur on a small scale in landslips and earthquakes, but that an entire country should execute a horizontal movement of translation is a supposition not warranted by recorded facts. That there may have been a time—or rather times—when frost was of rare occurrence in north-western Europe is highly probable. When France, Britain, and Germany displayed a semi-tropical flora, and when even Iceland, Lapland, and Spitzbergen had their forests of beeches and oaks, frost would doubtless be of rare occurrence; but there is no evidence that a period of this kind fell so recently as 2190 B.C., or, in other words, 4000 years ago. Here, then, is a decidedly mythological feature—the transposition of an event from its own to a later date.

The account of the "bad time" is, further, inconsistent, for though frosts now occur over most of the countries claimed as parts of the old Frisian dominion, yet wheat still ripens in every one of the countries specified, with the exception, perhaps, of Scandinavia. As to a heat sufficient to bake the wheat in the fields, we need scarcely say that since the earth became habitable no such temperature ever existed. On the other hand the "bad time" consisted in an alteration of the surface of the earth; portions of land were submerged, such as Atland, and in their stead land arose where formerly sea had existed. The case, therefore, was neither subsidence nor elevation, but a combination of both, and could not have been produced by a change in the position of the earth's centre of gravity, and a consequent transference of the main volume of the waters from one hemisphere to the other. Had it fallen two centuries earlier it might possibly have been identified by divines with the Noachian deluge.

The geography of the story is also somewhat obscure. If the Frisians occupied the country "to the extremity of the East Sea" and the banks of the Rhine from one end to the other, where was Twisklanden, the country of the

Germans, with its dense forests? It is also remarkable that the Frisians settled in the higher marches bordering on Germany—consequently to the east of Friesland—"were always" armed against the savage Britons (Vrwilderda Britne). How could the Britons attack Friesland from this side?

The following passage seems to prove that Dr. Ottema has not studied very carefully the book for which he stands sponsor:—"With regard to mythology this writing, which bears no mythical character, is not less remarkable than with regard to history, Notwithstanding the frequent and various relations with Denmark, Sweden, and Norway, we do not find any traces of acquaintance with the Northern or Scandinavian mythology. Only Wodin appears in the person of Wodan, a chief of the Frisians, who became the son-in-law of one Magy, king of the Finns, and after his death was deified." Yet on the very next page we read that Frya, the grand-daughter of the eternal and almighty Wr-alda* (query, *Ur-alte*, the primeval one), is the mother of Frya's people, the Frisians, and is reverenced as the representative of Wr-alda. Can Dr. Ottema have overlooked that this is none other than the Scandinavian Venus, Freya, or Friga, to whom the sixth day of the week was consecrated by our forefathers? Again, we read in the "Oera Linda Book" of Irtha, the earth, daughter of Wr-alda, and mother of Frya. Is not this the Hertha who was worshipped by the Germans, and whose consecrated chariot was preserved in the Isle of Rügen down to the times of Charlemagne? Then we have mention of Walhallagera as a city of importance. Does not this remind the reader of Valhalla, the gara or gardt (citadel) of the gods?

But whilst Dr. Ottema thus ignores the very palpable connection between his book and the Teutonic mythology, he finds in it the "closest connection" with Greek and Roman legends, and even derives from it an account of the "origin of two deities of the highest rank, Min-erva and Neptune. Minerva (Athene) was originally a Burgtmaagd, priestess of Frya, at the town of Walhallagara, Middelburg, or Domburg, in Walcheren. The other, Neptune, called by the Etrurians Nethunus, the god of the Mediterranean Sea, appears to have been, when living, a Friesland Viking, or sea-king, whose home was Alderga (Ouddorp, not far from Alkmaar). His name was Teunis, called familiarly by his followers Neef Teunis, or Cousin Teunis, who had chosen the Mediterranean as the destination of his expeditions, and must have been deified by the Tyrians at the time when the Phenician navigators begun to extend their voyages so remarkably, sailing to Friesland (!) to obtain British tin, northern iron, and amber from the Baltic, about 2000 years before Christ.

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* Named also, in one passage, Alvader, *i.e.*, All-father, the true name of the Supreme among the Teutonic nations.

"Besides these two, we meet with a third mythological personage—Minos, the law-giver of Crete, who likewise appears to have been a Friesland sea-king; Minno, born at Lindacord, between Wiesingen and Kreyl, who imparted to the Cretans an 'Asagaboek.' He is that Minos who, with his brother Rhadamanthus and Aeacus, presided as judge over the fates of the ghosts in Hades, and must not be confounded with the later Minos, the contemporary of Ægeus and Theseus, who appears in the Athenian fables.

"The reader may perhaps be inclined to laugh at these statements, and apply to me the words that I myself have lately used, fantastic and improbable. Indeed at first I could not believe my own eyes, and yet, after further consideration, I arrived at the discovery of extraordinary conformities, which render the case much less improbable than the birth of Min-erva from the head of Jupiter, by a blow from the axe of Hephaestus, for instance."

If the reader has managed so far to restrain his laughter we fear that his gravity will be completely upset by the exquisite simplicity of the sentence last quoted. Can Dr. Ottema really think that any one in these days conceives of Minerva as a veritable personage born from the brain of Jupiter? Does he so ill comprehend the nature of the myth as to think his own story must find acceptance because historically "less improbable"? If Minerva was a Frisian priestess who settles in Attica and builds Athens, how comes it that her name should survive in Italy and be replaced in Greece by Pallas? How, if Pallas came from Friesland, is she likely to have endowed Attica with the olive? "Cousin Teunis," as the etymology of Neptune, strongly reminds us of Dean Swift's derivation of the name Achilles, from "a kill ease." Indeed we are not without suspicions that the entire "Oera Linda Book" may be a solemn Dutch joke, and that the learned Doctor is gravely chuckling at his believing readers. We are somewhat startled to read of the Phenicians sailing to Friesland in order to obtain British tin. We know, from the most positive facts, that they came direct to Cornwall, where coins, local names, and evident traces of Phenician blood—as pointed out by Dr. Knox and other ethnologists—attest their former presence. For them to sail past Britain to Friesland for a British commodity would be as absurd as for a British ship in quest of tea to sail past China to Jesso.

One name occurring in the book might, we think, have been utilised by Dr. Ottema. The Priestess Adela says—"I refused to be Volcksmœder because I wished to marry Apol." We think that, with a little trouble and a few bold conjectures, this Apol might be identified with Apollo, even though the "God of life, and poetry, and light" is generally represented as having been a bachelor.

Neptune—we beg pardon, Cousin Teunis—had, it seems, a cousin and fellow-adventurer, one Inka. On one of their expeditions to the Mediterranean, after touching at Kadik (Cadiz), then a Frisian colony, they disagreed. "Teunis wished to sail through the Straits to the Mediterranean Sea, and enter the service of the rich Egyptian king, as he had done before, but Inka said that he had had enough of all those Finda's people. Inka thought that perhaps some high-lying part of Atland might remain as an island, where he and his people might live in peace. As the two cousins could not agree, Teunis planted a red flag on the shore and Inka a blue flag. Every man could choose which he pleased, and to their astonishment the greater part of the Finns and Magyars followed Inka, who had objected to serve the kings of Finda's people. When they had counted the people, and divided the ships accordingly, the fleet separated. We shall hear of Teunis afterwards, but nothing more of Inka."

We fear that Dr. Ottema has here, again, lost an opportunity. Is it not self-evident—to the enthusiastic at least—that Inka must have sailed away to the west, and discovered South America, where he founded the Peruvian Empire, which his descendants long ruled under the name of the Incas? Is it not, however, strange that so maritime a people as the Frisians, 180 years after the submergence of Atland, are represented as still in doubt whether any part of that region had remained as an island?

We further learn that Marseilles and Tyre were originally Frisian colonies, as well as Cadiz and Athens. From the latter they were ultimately expelled by Cecrops, a Frisio-Egyptian half-breed, represented as "bright of eye, clear of brain, and enlightened of mind." This leads us to one of the strangest passages in the entire work:—"Geert, the Frisian priestess of Athens, departed with the best of Frya's sons and seven times twelve ships. Soon after they had left the harbour they fell in with at least thirty ships coming from Tyre, with women and children. They were on their way to Athens, but when they heard how things stood there they went on with Geert. The sea-king of the Tyrians brought them through the Strait, which at that time ran into the Red Sea (now re-established as the Suez Canal). At last they landed at the Punjab, called in our language the Five Rivers, because five rivers flow together into the sea. Here they settled, and called it Geertmania. The King of Tyre afterwards, seeing that all his best sailors were gone, sent all his ships with his wild soldiers to catch them, dead or alive. When they arrived at the Strait both the sea and the earth trembled. The land was upheaved, so that all the water ran out of the Strait, and the muddy shores were raised up like a rampart. This happened on account of the virtues of the Geertmen, as every one can plainly understand." Thus it would appear that at one time the Red Sea communicated with the Mediterranean, and that Africa was an island, but that in B.C. 1551, or about that time, the passage was closed up. If the exodus of the Israelites out of Egypt is taken at the date ordinarily assigned to it, viz., B.C. 1564, we shall see that this region within some thirteen years witnessed the flight of two nations, each saved by a miracle of a converse nature. The Israelites, fleeing on land, are preserved by the influx of waters; the Frisians, escaping by sea, are saved by a rampart of land upheaved behind them! This is, to say the least, a very curious coincidence. Dr. Ottema very zealously supports this view of the Frisian colonisation of India. He says in his Introduction:—

"The historians of Alexander's expedition do not speak of Frisians or Geertmen, though they mention Indoscythians, thereby describing a people who live in India, but whose origin is in the distant unknown North.

"In the accounts of Lindgert no names are given of the places where the Frisians lived in India. We only know that they first established themselves to the east of the Punjab, and afterwards moved to the west of these rivers. We find in Ptolemy, exactly 24° N. on the west side of the Indus, the name Minnagara; and about 6 degrees east of that, in 22° N., another Minnagara. This name is pure Fries, the same as Walhallagara, Folsgara, and comes from Minna, the name of an Eeremœder (high-priestess), in whose time the voyages of Teunis and his nephew Inca took place.

"This coincidence (!) is too remarkable to be accidental, and not to prove that Minnagara was the head-quarters of the Frisian colony. The establishment of the colonists in the Punjab in 1551 before Christ, and their journey thither, we find fully described in Adela's book, and with the mention of one most remarkable circumstance, namely, that the Frisian mariners sailed through the Strait which in those times still ran into the Red Sea."

"In Strabo (Book I., pp. 38 and 50) it appears that Eratosthenes was acquainted with the existence of the Strait, of which the later geographers make no mention. It existed still in the time of Moses (Exodus xiv., 2), for he encamped at Pi-ha-chiroht, the 'Mouth of the Strait.' Moreover, Strabo mentions that Sesostris made an attempt to cut through the isthmus, but that he was not able to accomplish it. That in very remote times the sea really did flow through it is proved by the result of the geological investigations on the isthmus made by the Suez Canal Commission, of which M. Renaud presented a Report to the Academy of Sciences, on the 19th June, 1856. In that Report, among other things, appears the following:—'Une question fort controversée est celle de savoir si à l'époque où les Hebreux fugaient de l'Egypte sous la conduite de Moïse les lacs amers faisaient encore partie de la Mer Rouge. Cette dernière hypothêse s'accorderait mieux que l'hypothèse contraire avec le texte des livres sacrés, mais alors il faudrait admettre que depuis l'époque de Moïse le seuil de Suez serait sorti des eaux.'"

This entire passage is remarkable for its loose method of reasoning. That at some remote period there may have been a connection between the Red Sea and the Mediterranean is perfectly possible; but the question here at issue is, did such Strait exist down to the time of Moses, and disappear thirteen years after? The expression "Mouth of the Strait" proves nothing, since it may signify a narrow sea closed at one end, like either of the two northern forks of the Red Sea. We think that everyone who reads the Book of Genesis without prepossession must conclude that in the time of the early Hebrew patriarchs the road from Palestine to Egypt did not involve the passage of an arm of the sea. The attempt of Sesostris, as mentioned by Strabo, proves nothing, since such a canal would be equally useful whether the isthmus had existed from time immemorial or was a mere recent upheaval. Nor can much weight be laid on the name of Minnagara. That such a name, as given through the medium of a Greek author, and probably modified, may be found capable of a Frisian etymology—involving, too, a proper name—is a coincidence too trifling to be of any weight. We cannot lay claim to any philological authority, but those most profoundly versed in the science of language declare that the languages of India and of Western Europe all belong to one great family, and point to one common root. Where, then, is the wonder if an Indian name is capable of receiving a Frisian etymology?

In the account of the visit of Ulysses to Holland there is one very peculiar circumstance. The Frisians throughout this book are represented pre-eminently as the seafaring people of the world—the only nation who could or might navigate the sea of Wr-alda, or the great *ocean*. Yet the ships of Ulysses are described as "finer than any that we possessed or had ever seen." This, too, after having been driven about for years in strange seas, where they must have been damaged and shattered, and where they could have found but few opportunities for refitting.

Among the further suspicious features of the "Oera Linda Book" we are struck with its manner of appealing to that national pride in which, despite his phlegma, Mynheer is no wise deficient.* To the foreigner, Holland seems generally—perhaps too generally—a mere swampy appendage of Germany which has accidentally acquired independence, and its language a mere vulgarised form of German, or, as it has been humorously described, "German spoken during a bad fit of sea-sickness." Even such an author as Jean Paul Richter could so far forget the honourable and splendid part taken by Holland during the sixteenth, seventeenth, and eighteenth centuries, in the development of science, as to characterise the Dutchman as a "cheap edition of the German, printed on inferior paper, and without illustrations." But to the Dutchman who receives Dr. Ottema's revelations his country is no mere fragment of Germany, but the relic of a vast region to which imagination supplies the only limit—the originator of alphabetical writing and of our arithmetical notation, the instructress and civiliser of Greece, of Hetruria, perhaps of Phenicia, great alike in arms, in legislation, and in commerce, and planting her institutions even as far as the Punjab! We must further notice the side-blows dealt at the three mighty neighbours of Holland. France has proved herself dangerous and aggressive alike under Louis le Grand and under the first Napoleon. Accordingly we find mention of the "dirty Gauls." Germany is naturally a cause of dread and suspicion. Hence the "Twisker" are stigmatised in advance as "robbers." England has outstripped Holland in navigation, commerce, and colonisation. Therefore the "Oera Linda Book" dolefully relates how the Frisians lost Britain, once their penal colony, and how their rule anticipated ours in the Punjab. Can we imagine anything more soothing to Dutch patriotism?

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* Witness the indignation excited by Washington Irving's "History of New Amsterdam." Within the lifetime of the present generation a mere casual reference to this elegant writer has been known, in Dutch Society, to bring on the same ominous silence as might be provoked in an English drawing-room by the introduction of some indecent topic.

Another remarkable and suspicious circumstance is the hostility to priests and priestcraft pervading the whole document, and appearing even in a note appended by a former possessor of the book. Liko, surnamed Over de Linda, in the year A.D. 803, is made to write as follows:—"Beloved successors, for the sake of our dear forefathers and of our dear liberty I entreat you, a thousand times, never let the eye of a monk look on these writings." Now, that monks have destroyed many documents which would have thrown a priceless light on history and ethnology needs not to be contested. Nor is it, perhaps, altogether incredible that this fact should have been recognised as early as the year A.D. 803. But we can scarcely imagine a general and systematic dread of priestcraft before ecclesiastical power was formally organised, and before its "alethophobia"—as a modern French writer terms it—had been developed. It is, moreover, strange that a nation whose government might be called a theocracy directed by priestesses should show such a repugnance to priests. This feature, we think, points not doubtfully to a very modern origin of the work.

Dr. Ottema insists that "there is a striking difference between this book and the Greek myths. The Myths have no dates, much less any chronology, nor any internal coherence of successive events. The untrammelled fancy develops itself in every poem separately and independently. The mythological stories contradict each other on every point. 'Les Mythes ne se tiennent pas' is the only key to the Greek mythology.

"Here, on the contrary, we meet with a regular succession of dates starting from a fixed period—the destruction of Atland, 2193 before Christ. The accounts are natural and simple, often *naïve*, never contradict each other, and are always consistent with each other in time and place. As, for instance, the arrival and sojourn of Ulysses with the Burgtmaagd Kalip at Walhallagara (Walcheren), which is the most mythical portion of all, is here said to be 1005 years after the disappearance of Atland, which coincides with 1188 years before Christ, and thus agrees very nearly with the time at which the Greeks say the Trojan war took place.

"Another remarkable difference consists in this, that the Myths know no origin, do not name either writers or relators of their stories, and therefore never can bring forward any authority; whereas in Adela's book for every statement is given a notice where it was found or whence it was taken. For instance—'This comes from Minno's writings—this is written on the walls of Waraburch—this is in the town of Frya—this at Stavia—this at Walhallagara.'

"There is also this, further. Laws, regular legislative enactments, such as are found in great numbers in Adela's book, are utterly unknown in Mythology, and indeed are irreconcilable with its existence. Even when the Myth attributes to Minos the introduction of law-giving into Crete, it does not give the least account of what the legislation consisted in. Also, among the gods of mythology there existed no system of laws. The only law was unchangeable Destiny and the will of the supreme *Zeus*."

Now, as regards the alleged non-mythical character of the "Oera Linda Book," we point out elsewhere some instances to the contrary which seem to have escaped Dr. Ottema's notice. But the entire cosmogony contained in the "Book of Adela's Followers" is mythical in the fullest sense of the word. Hertha gives birth to three maidens:—

"Lyda out of fierce heat.
"Finda out of strong heat.
"Frya out of moderate heat.

"When the last came into existence Wr-alda breathed his spirit upon her, in order that men might be bound to him. As soon as they were full-grown they took pleasure and delight in the visions of Wr-alda.

"Hatred found its way among them. They each bore twelve sons and twelve daughters—at each Jule-time a couple. Thence come all mankind."

This surely is of the very essence of mythology, as is the next following account of the apotheosis of Frya and her transfer to a watch-star. But admitting that the "Oera Linda Book" differs in some points from the majority of mythological legends, we must draw from this circumstance a conclusion widely differing from that of Dr. Ottema. The references to authorities and the exact dates are, in our opinion, no mean testimony in favour of its being a supposititious production.

Meantime we cannot admit that entire freedom from contradictions for which Dr. Ottema contends. On p. 33 we read :—" In early times almost all the Finns (in the original Finda's folk) lived together in their native land, which was called Aldland, and is now submerged. They were thus far away, and we had no wars. When they were driven hitherwards, and appeared as robbers, then arose the necessity of defending ourselves, and we had armies, kings, and wars."

Yet, judging from other portions, Aldland or Atland was the patrimony of the Frisians, and its submergence " occasioned a great dispersion of the Frisian race." Moreover, if it was land stretching out to the west of Jutland, and if Heligoland and the isles of North Friesland, are its " last barren remains," it could in no manner be pronounced " far away," having been part and parcel of what is now known as Holland. Indeed the account of the origin of the Finns or Finda's folk, as given in detail on p. 73, fully contradicts the view of their having been the original inhabitants of Aldland :—

" One hundred and one years *after the submersion of Aldland* a people came *out of the East.* That people was driven by another. Behind us, in Twiskland (Germany), they fell into disputes, divided into two parties, and each went its own way. Of the one no account has come to us, but the other came in the back of our Schoonland (Skenland, or Scandinavia), which was thinly inhabited, particularly the upper part. Therefore they were able to take possession of it without contest, and as they did no other harm we would not make war upon them. They were not wild people like most of Finda's race, but, like the Egyptians, they have priests and also statues in their churches. The priests are the only rulers, and call themselves Magyars and their head man Magy : he is high priest and king in one. The rest of the people are of no account, and in subjection to them. This people have not even a name, but we call them Finns (Finna) because, though all the festivals are mournful and bloody, they are so formal that we are inferior to them in that respect." This passage decidedly proves the contradiction to which we have just referred.

As regards the coincidences upon which Dr. Ottema lays such emphasis, they are striking only if we start with the assumption that the book is a genuine document. If we consider it a forgery, they as decidedly support that hypothesis. Besides, it must be noted that the work gives us, after all, merely what may be called modified versions of old stories. There is nothing entirely new ; nothing which might not have been invented by an accomplished literary forger of the present day. We might naturally have expected that a people so civilised as the early Frisians are represented, even 2000 years before the Christian era, would have had something definite to communicate concerning their own origin and early history. But except the mythical account of their descent from Frya, and the laws received from her by revelation, we are left greatly in the dark. Whence came the Frisians? Were they truly aborigines, created or evolved upon the spot? We feel the more disposed to ask this question because, if the "Oera Linda Book" is to be trusted, our present views on the history of civilisation and on ethnology must be reconsidered. This work decidedly combats the prevailing view that Europe received its civilisation from Asia, culture being gradually extended westwards. On the contrary, it represents Eastern Europe and Western Asia as receiving arts, letters, and laws from the North-west. Surely before views thus contrary to well-known facts can be ascertained, the strictest scrutiny must be demanded.

There is a further consideration drawn, not from the contents of the book, but from its alleged history. Had the manuscript been lying, overlooked and forgotten, in the library of some convent or university, or in the muniment-room of some old or rarely-visited castle, its resurrection in the latter part of the nineteenth century would not have been, *prima facie*, improbable; but it is described as in the custody of a private family who knew of its existence and ascribed to it no little importance. It is specially bequeathed to the present holder by his grandfather. And all this in a highly educated country which has been for centuries honourably distinguished for its intellectual activity, and where men of learning abound! Can we imagine the possessors of this mysterious document feeling no curiosity as to its contents? Would they not naturally have consulted some archæologist or philologian, and would not the world long ago have been in possession of the secret?

That the book is interesting, and that the sentiments expressed are of a highly moral—we might almost say of a suspiciously moral—tendency, there is no occasion to dispute.

What may be the true origin of the "Oera Linda Book" we do not profess to decide. It may be a romance, a hoax, or an intentional and downright imposition. Or it may be a *saga* adroitly interpolated and manipulated. But we certainly cannot accept it as a genuine and authentic historical record.

Concerning the translation little need be said. Mr. Sandbach thinks that if the book is allowed to be 100 or 150 years old, there is no reason for refusing it a greater antiquity. He thinks there is "nothing in the narratives of this book inconsistent with probability," and he endorses the absurd remark of Dr. Ottema that it is not more improbable for a "clever woman to have become a lawgiver at Athens than for a goddess to spring, full-grown and armed, from the cleft skull of Jupiter!" *Sancta simplicitas!*

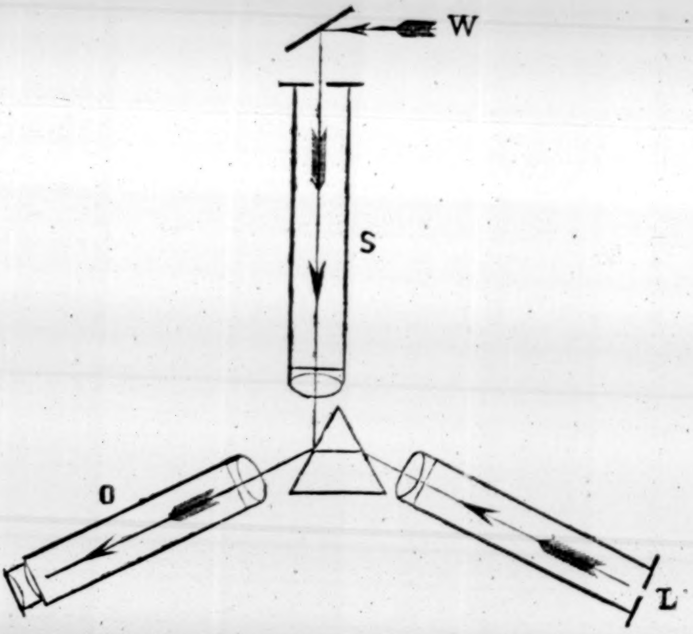
III. THE CONSTANTS OF COLOUR.

By Professor O. N. Rood, of Columbia College.

THE tints produced by Nature and Art are so manifold, often so vague and indefinite, so affected by their environment, or by the illumination under which they are seen, that at first it might well appear as though nothing about them were constant; as though they had no fixed properties which could be used in reducing them to order, and in arranging in a simple but vast series the immense multitude of which they consist.

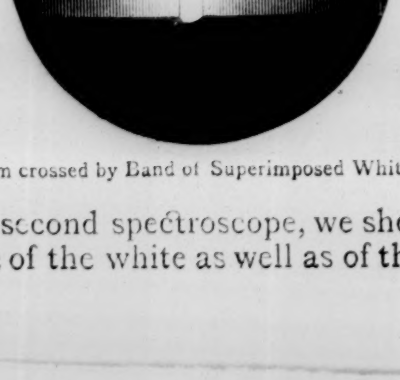
Let us examine the matter more closely. We have seen that when a single set of waves acts on the eye a colour-sensation is produced, which is perfectly well defined, and which can be indicated with precision by referring it to some portion of the spectrum. We have also found that when waves of light having all possible lengths act on the eye simultaneously, the sensation of white is produced. Let us suppose that by the first method a definite colour-sensation is generated, and afterward by the second method the sensation of white is added to it; white light is added to or mixed with coloured light. This mixture may be accomplished with an ordinary spectroscope, by removing the scale from the scale-telescope, and replacing it by a vertical slit, as indicated in Fig. 1, which is a view from above. Then if white light be allowed to enter this slit, it will be reflected from the surface of the prism into the observing-telescope, and we shall find that the spectrum is crossed by a vertical band of white light. By moving with the hand the scale-telescope, this white band may be made to travel slowly over the whole spectrum, and furnish us with a series of mixtures of white light with the various prismatic tints. (See Fig. 2.) The general effect of this proceeding will be to diminish the action of the coloured light; the resultant light will indeed present to the eye more light, but it will appear paler; the colour-element will begin to be pushed into the background. Conversely, if we now should subject our mixture of white and coloured light to analysis by a second spectroscope, we should infallibly detect the presence of the white as well as of the coloured light;

FIG. 1.



O is the observing-telescope; S, the scale-telescope; L, the source of light which furnishes the spectrum; W, the white light which is projected on the spectrum.

FIG. 2.



Spectrum crossed by Band of Superimposed White Light.

or, if no white light were present, that would also be equally apparent.

Taking all this into consideration, it is evident that when a particular colour is presented to us we can affirm that it is perfectly pure, viz., entirely free from white light; or that it contains mingled with it a larger or smaller proportion of this foreign element. This furnishes us with our first clue toward a classification of colours: our pure standard colours are to be those found in the spectrum; the coloured light coming from the surfaces of natural objects, or from painted surfaces, we must compare with the tints of the spectrum. If this is done, in almost every case the presence of more or less white light will be detected; in the great majority of instances its preponderance over the coloured light will be found quite marked. To illustrate by an example:—If white paper be painted with vermillion, and compared with a solar spectrum, it will be found that it corresponds in general tone with a certain portion of the red space; but the two colours never match perfectly, that from the paper always appearing too pale. If, now, white light be added to the pure spectral tint, by reflecting a small amount of it into the observing-telescope, it will become possible to match the two colours, and, if we know what proportion of white light has been added, we can afterward say that the light reflected from the vermillion consists, for example, of 80 per cent of red light from such a region of the spectrum, *plus* 20 per cent of white light. If we set the amount of light reflected by white paper as 100, then a surface painted with "emerald-green" reflects about 8 parts of white light; artificial ultramarine, 2 or 3 parts; red-lead, 7 or 8, &c. Some white light is always present: its general effect is to soften the colour and reduce its action on the eye: when the proportion of white is very large, only a faint reminiscence of the original hue remains; we say the tint is greenish grey, bluish grey, or reddish grey. The specific effects produced by the mixture of white with coloured light will be considered hereafter; it is enough for us at present to have obtained an idea of one of the constants of colour, viz., its purity. The same word, it may be observed, is often used by artists in an entirely different sense: they will remark of a painting that it is noticeable for the purity of its colour—meaning only that the tints in it have no tendency to look dull or dirty, but not at all implying the absence of white or grey light.

Next let us suppose that in our study of these matters we have presented to us for examination two coloured surfaces, which we find reflect in both cases eight-tenths red light and two-tenths white light. In spite of this the tints may not match, one of them being much brighter than the other, containing, say, twice as much red light and twice as much white light; having, in other words, twice as great brightness or luminosity. The only mode of causing the tints to match will be to expose the darker-coloured surface to a stronger light, or the brighter surface to one that is feebler. It is evident then, that brightness or luminosity is one of the properties by which we can define colour; it is our second colour constant. This word luminosity is also often used by artists in an entirely different sense, they calling colour in a painting luminous, simply because it recalls to the mind the impression of light, not because it actually reflects much light to the eye. The term "bright colour" is sometimes used in a somewhat analogous sense, but the ideas are so totally different that there is little risk of confusion.

The practical determination of the second constant is possible in a great many cases; it presents itself always in the shape of a rather troublesome photometric problem, capable of a more or less accurate solution. The relative brightness of the colours of the solar spectrum is one of the most interesting of these problems, as its solution would serve to give some idea of the relative brightness of the colours which, taken together, constitute white light. Quite recently a set of measurements were made in different regions of the spectrum by Vierordt, who denoted the points measured by the fixed lines, as is usual in such studies.* The following table will serve to give an idea of his results:—

Colour.	Degree of Luminosity.
Dark red	800
Red	4,930
Red, slightly orange	11,000
Orange-red	27,730
Orange	69,850
Yellow	78,910
Green	30,330
Cyan blue	11,000
Blue	4,930
Ultramarine blue	906
Violet	359
,,	131
,,	58
,,	9

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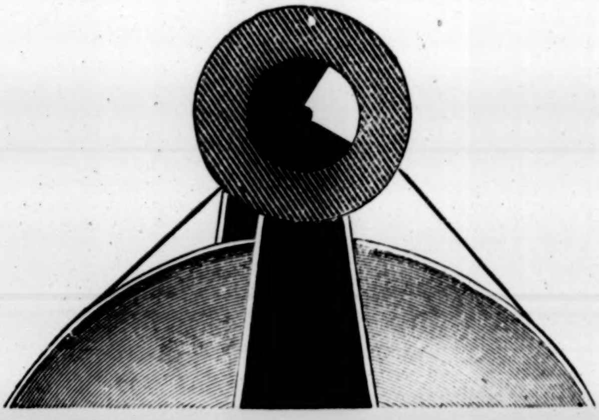
* C. Vierordt, Poggendorff's Annalen, Band cxxxvii., S. 200.

These measurements were made on a spectrum obtained by a glass prism, which, as has been mentioned previously, contracts the red, orange, and yellow spaces unduly, and hence increases their illumination disproportionately. It is to be hoped that a corresponding set of measurements will soon be made on the normal spectrum, furnished by a ruled plate. If we should multiply the luminosity of the colours in either kind of spectrum by their extent or areas, we should obtain measures of the relative amounts of these several tints in white light.

Fig. 3.



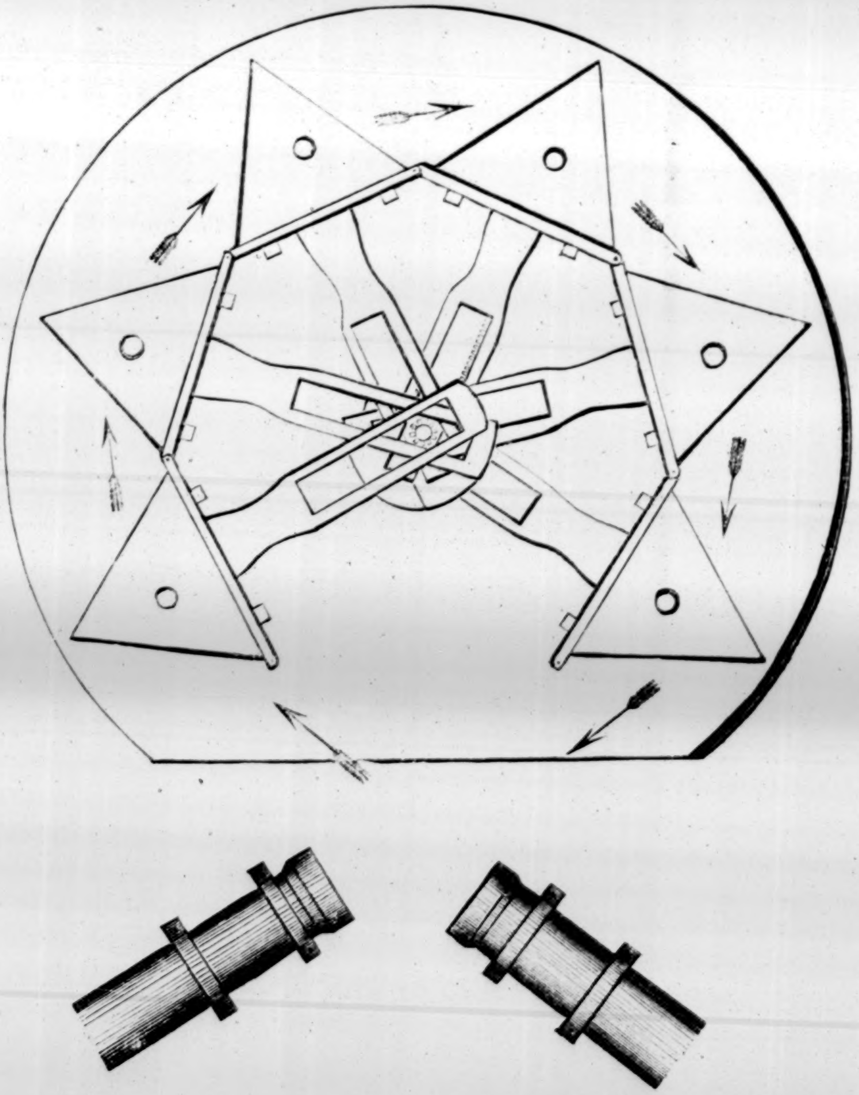
Coloured Disc with Small Black-and-White Disc.

Fig. 4.

Coloured Disc with Small Black-and-White Disc in Rotation.

By the simple method of rotating discs we can very roughly determine the second constant in the case of a coloured surface, for example, of paper tinted with vermillion. A circular disc, about 6 inches in diameter, is cut from the paper, and placed on a rotation apparatus, as indicated in Fig. 3. On the same axis is fastened a double disc of black-and-white paper, so arranged that the proportions of black and white can be varied at will. When the whole is set in rapid rotation, the colour of the vermillion paper will of course not be altered, but the black and white will blend into a grey. This grey can be altered in its brightness, till it *seems* about as luminous as the red. If we find, for example, that with the disc three-quarters black and one-quarter white, an equality appears to be established, we conclude that the luminosity of our red surface is 25 per cent of that of white paper. This of course based on the supposition that the black paper reflects no light; it actually reflects from 2 to 5 per cent, the reflecting power of white paper being put at 100. The results thus obtained are always inexact, and the same observer will often obtain

Fig. 5.



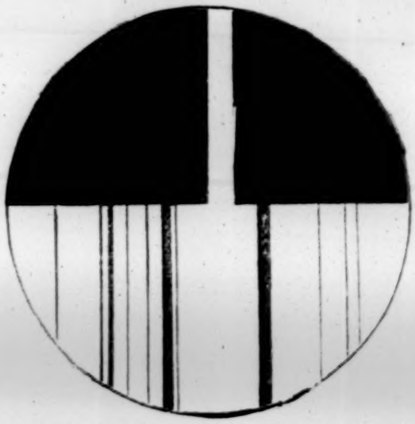
Facsimile of Rutherford's Drawing of Six-Prism Spectroscope.—(*American Journal of Science and Arts*, 1865.)

different results on different days, though those of a single day may agree pretty well among themselves. In the appendix a peculiar photometer will be described, which has been contrived by the author for the purpose of comparing more accurately together the relative luminosity of different coloured surfaces, or that of coloured and white surfaces.

But to resume our search for colour-constants. We may meet with two portions of coloured light, having the same degree of purity and the same apparent brightness, which nevertheless appear to the eye totally different; one may excite the sensation of blue, the other that of red; we say the *tones* are entirely different. The *tone* of the colour is then our third and last constant, or, as the physicist would say, the degree of refrangibility, or the wave-length of the light. It has previously been shown that the spectrum offers all possible tones except the purples, well arranged in an orderly series; and the purples themselves can be produced with some trouble, by causing the blue or violet of the spectrum to mingle in certain proportions with the red. Rutherford's automatic six-prism spectroscope can very conveniently be employed for the determination of the tone. (See Fig. 5.) A peculiar eye-piece is to be used, which isolates a little slice of the spectrum in its upper half, as indicated in Fig. 6. In the lower half of the field the fixed lines are seen, and the tone selected as matching the colour under examination can be located by their aid. Afterward, if it is considered desirable, white light can be added to the spectral tint, till it is subdued sufficiently to render exact comparison possible.

The experimental determination of the colour-constants is beset with a considerable amount of difficulty, even in the simplest cases, such as cardboards covered with pigments. The best mode of proceeding appears to be to call the luminosity of white cardboard 100, and then to determine photometrically the comparative luminosity of the coloured cardboards. The measurement of the amount of white light reflected along with the coloured is still more troublesome, and the result likely to be somewhat less exact, while the determination of the tone, or third constant, is moderately easy under favourable circumstances. One of the uses of such determinations is, the production of a set of standard coloured discs with known constants, which can afterward be combined with each other, as well as with standard black or white discs, so as to generate at will, with ease and certainty, an immense number of tints whose constants will be known. If we make a record of the constants involved in such experiments we can afterward reproduce the tints just as they originally were, or alter them to any desirable extent. To carry out the letter of this it will of course be necessary to view the standard discs under similar illuminations at different times—a point which can be secured with the aid of the photometer above referred to. The standard discs can also be used for building up a set of standard charts, containing a vast variety of tints of known composition, arranged methodically with regard to purity, luminosity, and tone. These matters will be considered at some length in a separate chapter, and are now only hinted at as a justification for the trouble we have been at in defining the constants of colour.

FIG. 6.



Eyepiece for Isolating the Tints of the Spectrum.

There is another point to be touched on in this connection. One of the most noticeable things about colours is their difference in *intensity*. Colours are intense when they excel both in purity and brightness; for it is quite evident that, however pure the coloured light may be, it still will produce very little effect on the eye if its total quantity be small; and, on the other hand, it is plain that its action on the same organ will not be considerable if it is diluted with much white light. Purity and brightness, or luminosity, are, then, the factors on which intensity depends. We shall see hereafter that this is strictly true only within certain limits, and that an inordinate increase of luminosity is attended with a loss of intensity of hue.

Having defined the three constants of colour, it will be interesting to inquire into the sensitiveness of the eye in these directions. This subject has lately been studied with care by Aubert, who made an extensive set of observations with the aid of coloured discs.* It was found that the addition of 1 part of white light to 360 parts of pure coloured light produced a change which was perceptible to the eye; smaller amounts failed to bring about this result. It was also ascertained that mingling pure coloured light with from 120 to 180 parts of white light caused it to become invisible, the hue being no longer distinguishable from white. Differences in luminosity as small as 1-120 to 1-180 could under favourable circumstances be perceived. It hence followed that irregularities in the illumination or distribution of pigment over a surface, which were smaller than 1-180 of the total amount of light reflected, could no longer be noticed by the eye. Experiments with red, orange, and blue discs were made on the sensitiveness of the eye to changes of tone or refrangibility: thus the combination of the blue disc with a minute portion of the red disc altered its hue by moving it a little toward violet; on reversing the case, or adding a little blue to the red disc, the tone of the latter moved in the direction of purple. Similar combinations were made with the other discs. Aubert ascertained, in this way, that recognisable changes of tone could be produced by the addition of quantities of coloured light as small as from 1-100 to 1-300 of the total amount of light involved. From such data he calculated that in a solar spectrum at least 1000 distinguishable tones are visible. But we can still recognise these tones when the light producing them is subjected to considerable variation in brightness. Let us limit ourselves to 1000 slight variations, which we can produce by gradually increasing the brightness of our spectrum, till it finally is ten times as luminous as it originally was. This will furnish us with a million tones, differing perceptibly from each other. If each of these tones is again varied 300 times, by the addition of different quantities of white light it carries up the number of hues we are able to distinguish as high as 300,000,000. In this calculation no account is taken of the purples, or of colours which are very bright or very faint, or mixed with very much white light. For these it will hardly be extravagant to demand another 100,000,000; we reach thus the astonishing conclusion that the human eye under favourable circumstances is able to distinguish as many as 400,000,000 different hues!—*From Advance Sheets of the New York "Popular Science Monthly."*

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* AUBERT, Physiologie der Netzhaut. Breslau, 1865.

IV. THE ENCOURAGEMENT OF SCIENTIFIC RESEARCH.*

AN opinion is rapidly gaining ground that the present scientific position of Britain is unsatisfactory, both as compared with that of certain foreign nations and with our own antecedents, and is consistent neither with the honour nor with the true interests of the Empire. To escape misunderstanding or misrepresentation we must endeavour to state the case, as it appears to us, with more precision. We do not deny that modern England can boast a series of names which, if equalled elsewhere, are surpassed nowhere. We believe that in *scientific ideas* we are on a level with the foremost nations of the world; that in speculative philosophy we have re-conquered the foremost place, whilst in biology we have initiated a complete regeneration. We have the greater pleasure in pointing out these saving considerations since they prove that our shortcomings are due not to any natural deficiencies in the British and Irish mind, but to circumstances fortuitously or artificially created, and which may of course be artificially modified. What we complain of, then, relates not to the height of our *scientific ideas*, but to the quantity of our *scientific work* and the number of our earnest and qualified *scientific workers*. On this point there is scarcely room for discussion. Even the most pseudo-patriotic John Bull who accuses us of undervaluing our own country can scarcely deny the facts which we have to bring forward. Let us look at our scientific literature; it is exceedingly rich in the mere number of books published; but what an overwhelming proportion of them—as every reviewer knows to his sorrow—are mere compilations, elementary treatises, and the like, well-known matter brought forward again and again in a slightly modified form. How many of the original works, even, are original in little save absurdity, and consist in wild attempts to subvert the whole existing system of our knowledge and rebuild it as if by magic. Let us turn to the original papers announcing some discovery of greater or less moment, and appearing in the Transactions or Journals of learned societies, and in the various scientific periodicals. Here our comparative poverty is most striking. Let us take up, for instance, the "Berichte" care by Aubert, who made an extensive set of observations with the aid of coloured discs.* It was found that the addition of 1 part of white light to 360 parts of pure coloured light produced a change which was perceptible to the eye; smaller amounts failed to bring about this result. It was also ascertained that mingling pure coloured light with from 120 to 180 parts of white light caused it to become invisible, the hue being no longer distinguishable from white. Differences in luminosity as small as 1-120 to 1-180 could under favourable circumstances be perceived. It hence followed that irregularities in the illumination or distribution of pigment over a surface, which were smaller than 1-180 of the total amount of light reflected, could no longer be noticed by the eye. Experiments with red, orange, and blue discs were made on the sensitiveness of the eye to changes of tone or refrangibility: thus the combination of the blue disc with a minute portion of the red disc altered its hue by moving it a little toward violet; on reversing the case, or adding a little blue to the red disc, the tone of the latter moved in the direction of purple. Similar combinations were made with the other discs. Aubert ascertained, in this way, that recognisable changes of tone could be produced by the addition of quantities of coloured light as small as from 1-100 to 1-300 of the total amount of light involved. From such data he calculated that in a solar spectrum at least 1000 distinguishable tones are visible. But we can still recognise these tones when the light producing them is subjected to considerable variation in brightness. Let us limit ourselves to 1000 slight variations, which we can produce by gradually increasing the brightness of our spectrum, till it finally is ten times as luminous as it originally was. This will furnish us with a million tones, differing perceptibly from each other. If each of these tones is again varied 300 times, by the addition of different quantities of white light it carries up the number of hues we are able to distinguish as high as 300,000,000. In this calculation no account is taken of the purples, or of colours which are very bright or very faint, or mixed with very much white light. For these it will hardly be extravagant to demand another 100,000,000; we reach thus the astonishing conclusion that the human eye under favourable circumstances is able to distinguish as many as 400,000,000 different hues!—*From Advance Sheets of the New York "Popular Science Monthly."*

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* Essays on the Endowment of Research. By Various Writers. London: H. S. King and Co.

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* Essays on the Endowment of Research. By Various Writers. London : H. S. King and Co.

The following evidence, given by Dr. Frankland before the Royal Commission on Scientific Instruction, is painfully significant*:—"A year or two ago I took the trouble to look out, in regard to chemistry, the number of original investigations made in each country during one year. In the year 1866, which was the year I enquired, 1273 papers were published by 805 chemists. Of these Germany contributed 445 authors and 777 papers; France, 170 authors and 245 papers; the United Kingdom, 97 authors and 127 papers. I may mention, however, speaking exclusively of chemistry,—for I have not gone into other sciences,—that, as far as research in Great Britain depends upon our scientific training, our case is much worse than appears from this comparison, because a large proportion of those papers contributed by the United Kingdom were the work of Germans residing in this country."

It is further remarked, in the work before us, that "from a schedule of original researches executed in the laboratory of the Royal College of Chemistry since 1845, and handed in by Dr. Frankland to the Commission, it appears that out of 140 specified researches no less than 70 were made by foreigners! That is to say, Germany not only produces four and a half times as many investigators in chemistry and six times as many researches in a year as we do, but actually produces half the number of researches which in this calculation are credited to this country. I may illustrate the truth of these comparisons by an analysis of the original works published in the West of Europe during a period taken at random, the first fortnight of September, 1873.

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* Report of Evidence, p. 371.

"In Physical Sciences I find that thirty-one treatises, almost all of them exhibiting special and original researches, were published. Of these nineteen are German, eight are French, two Italian; while England is represented by Thorpe's "Manual of Quantitative Analysis" and by a popular work on the moon!

"In History the proportion is much more favourable to England, *thanks to the practical endowment of historical research at the Rolls Office*. Of fourteen books four are German, four English, two French, and four belong to various other nations."

This certainly has a most alarming sound, and if we had no other evidence to bring forward we might, on the faith of this alone, lay claim to have proved our proposition that our scientific position, as a nation, is most unsatisfactory. But this is far from all. If we turn from chemistry to other branches of science—to physics, biology, geology, philology —we still find the case in the main unaltered. Our original scientific work is far surpassed in its amount by that of France and Germany, and our competent scientific workers are surpassed in numbers.

Is not the subjoined case truly humiliating? "The fungus which produces the potato-disease is still imperfectly known, and the Royal Agricultural Society—with a laudable desire to remedy that deficiency, in the absence of any laboratory in this country where such investigations are prosecuted—was compelled to have recourse to that of the University of Strasburg, and placed £100 at the disposal of Professor De Bary, in order to have the subject completely investigated."

We come now to a part of the subject which requires a somewhat delicate treatment, but which is far too serious to be passed over. Surely if there is any one thing which we should produce in sufficient amount for our own wants, and which we ought rather to export than import, it is disciplined intellect. To bring into England foreign thinkers, discoverers, improvers, ought to be as superfluous and unremunerative a task as bringing coals to Newcastle. But turning from what should be to what actually is, we must tell a different tale. How many professorships in our universities and colleges are now held by foreigners? How many places of trust in museums, libraries, botanical gardens, and the like, are not in alien hands? How many foreign chemists, and even engineers, are now either in private practice throughout the kingdom or hold the most lucrative and responsible positions in our manufacturing establishments?

On the other hand, is there in France or Germany a single professorial chair—other than of the English language and literature—or any similar position held by a British subject? In India and the Colonies the case is very similar to what it is in the home kingdoms. Now these phenomena are not without cause. We make all due allowance for that decay of national feeling, that morbid cosmopolitanism, which, on the principle that "extremes meet," is generally the outcome of the most intense personal selfishness, and which has been so blatantly preached and so widely practised in England; we know, too, that it flatters the pride of many a *nouveau riche* amongst us to be able to inform his friends that he has German or French chemists in the laboratory of his works, and Italian artists in his designing-room. We are aware that in an unorganised profession like that of chemistry many a stranger, who could not otherwise obtain a foot-hold, contrives to enter by underbidding his native competitors, or even by the unprofessional stratagem of giving his services for a year gratuitously. Nor—low be it spoken! —do we forget that foreign men of science in this country have enjoyed the benefit of exalted patronage. But when all due weight is given to these various considerations we fear the fact remains that French and German men of science are able to take root in this country because they, in many cases, bring to their tasks a more thorough training than do their English rivals. This consideration is to us a very "handwriting on the wall." Is this nation to live, like the dogs, on the intellectual crumbs that fall from the tables of France and Germany? Are we to commit the higher education of our youth, the supervision of the most delicate processes in our manufactures, and the conception and introduction of improvements to aliens, and to allow our own people to subside into the condition of hewers of wood and drawers of water? Is it of any avail that a certain abstract England shall grow more and more rich and prosperous, if it is to be by the degradation of the English people? Can we hope for any length of time to compete with rival nations if we have to borrow from them the intelligence needed to carry on our operations? Alien inventors and managers are in these days no less dangerous than mercenary troops, and if we are content to hire talent from abroad instead of cultivating it at home we are surely preparing our own downfall. Need we recall the fact that the Norman Conquest had been prepared during the reign of Edward the Confessor, by an influx of Norman ecclesiastics, Norman courtiers, and even Norman artizans? Are there not certain phenomena in our day far too closely analogous?

But, returning from this digression, we may surely consider ourselves fully warranted in concluding, from the facts brought forward, that England is not playing a worthy and a creditable part in the increase of human knowledge. We may infer, also, as a corollary, that by such neglect and backwardness her industrial and commercial pre-eminence, her power, and possibly even her very existence as the focus of a mighty Empire, may be gravely endangered.

We have next to enquire what are the causes of the evil which we have just recognised, and which we all, more or less, agree to deplore. We have already stated our opinion that if scientific research does not flourish amongst us as it ought, this is due not to any deficiency in intellect, not to any idiosyncrasy in the national mind, not to indolence or indifference to truth, but simply to the fact that in obedience to an unfortunate complication of circumstances our energies and our intellects have been turned into other channels. Till lately our educational course, both in schools and colleges, has been almost exclusively literary, whilst the subjects popularly known under the name of Science have been totally and contemptuously excluded. With a curious inconsistency they were denounced by men of business as useless, because not conducive to immediate gain; and branded by school-pedants as low, mechanical, and illiberal, because capable of application at all! Even now, in spite of the change which has taken place in public opinion, only ten endowed schools in England give even a poor, pitiful, four hours weekly to the study of science. A contemporary justly remarks—"In a neighbourhood of rural squires and clergy, untempered by a large town's neighbourhood, and unchecked by any man of education and intelligence holding sovereignty by virtue of superior rank and wealth, a school which treads doggedly in the ancient paths will certainly succeed even in second-rate hands, while a school which under superior chieftainship asserts the claims of science, and whose theology is therefore suspicious, will as certainly struggle long for existence, if it does not finally succumb."

Passing from our public schools to the universities, especially the two great and time-honoured institutions of Oxford and Cambridge, we see little on which we may congratulate ourselves. Physical science has, indeed, found a place along with mathematics and classics,—not, however, as a subject to be cultivated and advanced, but merely as a something to be examined in. A more fatal mistake it would be difficult to conceive. "Competitive examinations and original research," as one of the writers of the book before us most aptly remarks, are "incompatible terms."

It may seem a bold assertion, but it is our deliberate opinion that the power of assimilating the views and ideas of others stands in no relation at all to the power of originating anything of value. Receptivity is no sign of fecundity. Nay, the very reverse is often the case—just as it is easier to introduce matter into any vessel the emptier we find it. The man who "crams" easily and quickly, and who shines in competitive examinations, will often be found to have an essentially unproductive mind, poor in resources and barren of suggestions. A. reads every book laid before him by his tutors, and implants its contents in his mind, without the slightest regard to their truth or falsehood, to their value or worthlessness, or to the collateral issues which they raise. B. pursues the exact contrary course; he reads critically; he follows out and tests the author's views, and his mind as he studies is filled with suggestions. We shall of course admit that B. is the true student, and that A. for all higher purposes is merely wasting his time. But the Board of Examiners judge differently; they "pluck" the thoughtful B., and pass the shallow pretentious A.—who certainly differs from a Strasburg goose in being devoid of feathers — in high honours.

But not merely does the system of competitive examinations encourage many of the worst men and reject many of the best. When it once entangles a man endowed with a naturally good intellect, and sincerely anxious for knowledge, it rarely fails to ruin him. The following passage describes the working of this fatal device with no less force than truth:—"The extinction of disinterested study is a necessary consequence of the encouragement of cram. When the best and most receptive years of a man's life have been passed in having the doctrine ground into him that the end of all reading is to cheat the examiner, and that knowledge is valuable only so far as it can be made to tell in an examination, it is hard to see how he can unlearn the teaching he has received, and alter the character that has been formed in him. The grown man is what he has been taught to be, and out of cram may come many pages of examination-answers, or even a fellowship, but not original research, and the love of knowledge for its own sake. The specialist at the universities finds himself a marked man with a wisp of hay upon his horns—he is looked upon with mingled feelings of suspicion and pity. That there can be any knowledge outside the curriculum of the university, or if there is that it is of any value, is seldom dreamed of. More exclusive than an oligarchy of birth, more sordid than an oligarchy of wealth, we assume that the only subjects worth learning are those in which we examine, and that the worth even of those consists in their being made to 'pay.' Professor Max Müller offered in vain term after term, to read the *Rig-Veda* with any one of the 2400 members of the University of Oxford; none would go to him, since a third hand acquaintance with a few words and forms from that oldest specimen of Aryan literature is sufficient for the schools. The same professor, one of the most fascinating of lecturers, when lecturing on the fascinating subject of comparative mythology, which he has made completely his own, could collect but a miserable fragment of an audience around him, and even of this the larger part consisted of college tutors who intended to retail to their own pupils some of the crumbs which had fallen into their note-books. It is almost impossible to find a majority of fellows in any college willing to give away a single fellowship for a special subject "not recognised in the schools," even when the candidate does not object to be examined; and after this "idle fellowships" are defended on the ground that they encourage study and give an opportunity for learned leisure. But the study and learning that are meant are the study and learning that grow up out of the questions and answers in an examination room. The specialist who pleads in behalf of another kind of learning is considered a fanatic out of harmony with the spirit of our English universities and unappreciative of their merits. "We don't want original researchers," I have not unfrequently heard it said, "but good all-round men"—that is to say, "the best specimens of the crammer who have a smattering of many things, but know nothing well." Did ever system pronounce more emphatically its own condemnation? Surely the humblest "original researcher," the man who discovers only one fact unknown before, is of more value to the world than all the "good all-round men" who have ever lived, or ever will live, till competitive examination is hunted back in contempt and loathing to its native China! "Examination is a bad test," flippantly exclaims a daily paper, "but can you suggest a better?" "Can you," it might be replied, "suggest or conceive of a worse?" But if the reader will have patience to follow us, we will suggest an infinitely better test.

But the system we are condemning ruins not the student but the teacher also. One of the writers in the work which has given rise to the present paper is struck with a remarkable difference between the Continental and the English colleges. The most brilliant and honoured names in German science, Woehler, the Roses, Mitscherlich, Bunsen, Hofmann, the great Liebig himself—the men who uphold the splendid intellectual reputation of their country, are university professors. In England, on the contrary, the professors, college lecturers, and by whatever other name our university teachers may be known, are not as a class distinguished for original research. Few of them, indeed, could or would even claim to rank with the representative men of British science. This broad distinction, involving as it does the admission that our professorial chairs at present do not form prizes for scientific distinction nor positions where science may be successfully cultivated, is considered by the writer as due to some difference in national character. We think we can give a more correct explanation. The German professor wins his chair by virtue of a reputation based on research. On the amount, the importance, and the success of his subsequent investigations depends the number of students who flock to his lecture hall, and, in the case of the physical sciences, who seek to work under his supervision. As a matter of course, his income, his standing with the Senate and with the Government rise in the same proportion. Decorations, titles of nobility, honours of every kind beckon him on. The most eminent universities outbid each other for his services. With such rewards held out, to be gained by research, and by that means only, is it any wonder that the German professor "works," even if he has not—which is rarely wanting, a sincere and intense love for its own sake? And not only is he diligent in his own person. He selects his more promising students, places his ideas in their hands, assists them with his advice and suggestions, and associates their names with his own in the published results. He thus makes his laboratory, whether physical, chemical, or biological, a very focus of healthy and intense scientific activity. At the risk of somewhat anticipating ourselves, we cannot help here remarking that Germany holds no exclusive patent right for arrangements which not only commend themselves to our common sense, but which have worked so well in actual practice.

The position of an English university professor is almost in all points the opposite. To students, whose great object is not to be initiated into actual research, whether scientific, literary, historical, or theological, not to hear novel and profound ideas fresh from the lips of the master-minds of the age, but merely to pass certain examinations better than their competitors, what is the use of the eminence of a professor? How many additional students has the reputation of Dr. Max Müller or of Dr. Odling drawn to Oxford, and how much would the number be increased if the merits and the achievements of these two distinguished men were multiplied tenfold? The matter lies in the proverbial nutshell. The English professor does not "work" because he has no adequate motive. Nay, we are far from sure that he has not very direct motives for idleness. In a university where "original researchers" are not wanted, it is very possible that a professor who should succeed in breathing an intense intellectual life into the dry bones around him, would not soon be made to feel extremely uncomfortable and out of place.

But if the universities fail to encourage, or rather if they pointedly discourage, original investigation—there are, it may be said, other establishments founded for the very purpose of training up students for actual scientific research. We will name, for instance, the School of Mines and the Royal College of Chemistry. It is far from our object to bring any charges against either the constitution or the management of these establishments, although we believe that they are guilty of one sin of omission in common with the universities. But we must draw an odious comparison. It is well known that at such establishments as the Polytechnic School of Aix-la-Chapelle, a sound scientific training such as will fit the recipient for a career of research, as well as for the practical application of physics, chemistry, &c., to the arts and manufactures can be had for about £10 to £12 yearly. A similar education in England will be found very much more costly.

But let us suppose that whether at school, at the university, or at the establishments last-mentioned, a young man has contrived to acquire a sound, scientific training. Let him possess the natural qualifications needful, and let his tastes urge him strongly in the direction of scientific inquiry. Shall he—can he—devote his life to the advancement of physics, chemistry, or biology? The answer is too ready. His friends will tell him bluntly, but unfortunately with perfect truth, that "research does not pay." Except, therefore, he possesses independent means, he must abandon his project, or, at the best, effect an unsatisfactory compromise. Of course, if research is to be practically confined to the few, who, with the needful faculties, tastes, and training, combine the advantages of fortune, we need not wonder that our national amount of scientific work is so limited. What still aggravates the evil is the morbid, wealth-worship dominant in England which actually censures as " idle " the man, however affluent, who does not devote his time to some money-making pursuit. The man content with a competence, in order that he may have full time to devote to some important pursuit, is not a common English phenomenon, and when met with he is more apt to incur ridicule and contempt than respect.

Research, too, whilst it affords its votaries neither wealth nor even the very means of existence, can offer no compensation in the way of honour. Titles and distinctions in this country are very strictly confined to the following classes: —Military and naval men, lawyers, and financiers who " threaten the ministry that they will withdraw their capital from the country unless placed in the social position to which they think themselves entitled." Occasionally an eminent physician or engineer may be rewarded with a knighthood or baronetcy,—more, however, in acknowledgment of the successful routine practice of his profession than of anything done for the extension of human knowledge, —and some few men of science in the strictest term, such as Davy, Brewster, and Murchison, may thus be honoured. Berzelius was ennobled in Sweden, Liebig in Germany, and Cuvier in France, but England refuses to follow this example, and has never, we believe, offered even the lowest grade in the peerage to the most distinguished man of science. Philosophers, we are told, ought to be above such considerations; yet philosophers have very much the same wants as other men. We may safely venture to say that had Newton, Davy, or Faraday been ennobled, the *status* of all scientific men in the kingdom would have been most distinctly raised, and their pursuits would henceforth have met with a much fuller appreciation.

But in addition to all these direct discouraging influences the tendency to scientific research is repressed in an indirect manner, viz., by the circumstances which powerfully tend to draw talent into other channels. The most formidable rival of science in this country is the law which—as regards social position, influence, and emolument—offers rewards far beyond anything that scientific research does or perhaps ever can offer. There are indeed those amongst us who think that we may be content, and accept our bar and bench as an adequate compensation for the want of such a phalanx of scientific worthies as Germany possesses. We cannot adopt this view. We do not see that any conceivable number of judges and barristers, endowed with even inconceivable ability and integrity, can add to our national resources or enable us to compete to more advantage with foreign rivals. Nor, if we turn from utilitarian considerations to the question of national honour, is the case altered. Strange as it may sound in some quarters, we will venture to say that Faraday alone has done more to raise England in the estimation of the civilised world than have all the most eminent counsel of the day. Hence we do not think that social arrangements which divert talent from the laboratory to the bar are conducive to the national welfare, or that their workings should be witnessed without regret.

Summing up this part of our subject, we may safely venture the opinion that—regard being had to the extremely unfavourable circumstances prevalent—the paucity of scientific research in the United Kingdom cannot excite our wonder, but that there is rather room for surprise and for self-congratulation that under such conditions so much has been accomplished. But there now remains the only useful part of our task—to point out how these circumstances and conditions may be so modified that the scientific intellect of England may meet with fair play. Research does not at present "pay." We must, then, show how it may be made to pay. How is the much needed "endowment of research" to be most satisfactorily effected? Before coming face to face with this question we must first, however, dispose of a few misconceptions which still find a lurking-place in a certain class of minds. Forth steps, for instance, a political economist, of a type very common in Chambers of Commerce, and proceeds to inform us that if research is really valuable it "ought to pay" those who carry it on, and that if it "does not pay" it cannot really be valuable. Our friend evidently forgets that this England of ours not being quite Utopia, things are not always exactly as they "ought" to be. Our worthy objector himself, for instance, "ought" not to adulterate his calicos with China clay, which is yet to be detected in them in large quantity. We are firm believers in the saying of Francis Bacon, that one scientific truth, once established, draws after it "whole squadrons" of utilities and practical operations. But all this does not prove that research, however successful, can be commercially remunerative. A principle established in this century may not find its practical application till the next. Yet are we, on this account, any the less indebted to the discoverer? A scientific investigator is like the first traveller in some unsettled land: he may note the existence of valuable minerals, he may recognise the fertility of the land, but he cannot stop to raise the ore, to clear the forests, and to plant crops. These tasks he leaves to his successors. But is he not all the while to be really credited with the practical benefits springing from his exploration? It is somewhat significant that the olive, the symbolical tree of Minerva, is of very slow growth: he who plants it benefits posterity. Not only do the advantages derived from research often come too late to benefit their author; they are sometimes lost to him from their very width. If we serve an individual man we have some notion where to look for a reward. If we serve a town, a community, or a nation, we may perhaps be told that what is everybody's business is nobody's. But we wish the nation to accept its responsibilities to its benefactors, and therefore to endow research. Nor can we presume to draw a boundary line between useful research and such as seems to be merely curious. The whole history of science warrants us in maintaining that the most abstract truths, to appearance utterly unconnected with the wants of our daily life, may suddenly prove of immense practical importance. Every discoverer of a new fact or a new law has therefore a certain claim, which it is our truest wisdom to honour.

Dismissing, then, this objection, and reminding our friend the economist that the value of anything to the world and its market value are not necessarily proportional, we encounter another protest. The "researcher"—this somewhat unclassical college word is after all useful—we are told may embody his results in a book, and thus make them remunerative. Now in some branches of investigation this is to a certain extent the case. Literary, historical, or especially theological, investigation may thus be rendered self-supporting. In a less degree this holds good with geological and biological research, especially when, as sometimes happens, blended with narratives of adventure, travel, and sport. In such cases a book decidedly scientific in its object may circulate among the outside public and may be "ready at all the libraries," to the benefit of the author. But in physics and chemistry there is no such expedient. A record of discovery in these departments will not make a "readable book." The public at large have not the remotest conception of the amount of labour expended in attaining apparently trifling results. A chemical formula, a mathematical expression—apparently trifling, though really of the utmost value to science—may often embody the whole result of years of anxious, minute, and difficult toil. To give a single instance. A friend of ours was engaged for nearly ten years with a chemical investigation. He expended during this time in apparatus and materials not less than a thousand pounds sterling. We are not prepared to deny that he elaborated and perfected means of research which will be useful on other occasions, and he "struck the trail" of collateral issues; but what may be called the direct result of his labour was finally expressed in a single line in the "Philosophical Transactions." Surely, then, to bid the chemist or the physicist seek to reimburse himself for his time and trouble by publishing the results of his researches is childish mockery!

Literature, in fact, offers but little scope for a scientific man. Even when a scientific book has to be criticised or a new hypothesis discussed the political and literary organs of the day rarely take the trouble of referring the matter to any specially qualified writer. They know full well that blunders on such subjects are not likely to be detected, however gross, and if detected are not looked upon as disgraceful. It is not long since a morning paper informed the world that all gases, if heated to a point far below redness, exploded, leaving nothing but "a vacuum and a few particles of dust."

The next fallacy we have to encounter is the notion that scientific research may be efficiently carried on by men engaged in professions or in business in the intervals of their ordinary occupation. Some even "out-Herod Herod" to the extent of maintaining that it is better for a scientific man not to have his whole time at his own disposal. We regret to find this absurdity insinuated in a recent biographical work. We do not, of course, deny that certain men have contrived to do valuable work in science, even although trammelled with a trade or a profession. But we hold it to be utterly indisputable—indeed, self-evident—that had their time not been broken into by business they would assuredly have done more. A swift and powerful horse may travel at a certain speed when yoked to a cart, but cut the traces and he will assuredly be able to cover more ground in the same time. It is perfectly true that Daniel Hanbury engaged successfully in chemico-pharmaceutical and botanical researches at the same time that he was at the head of a business establishment. But we must remember that his case was exceptional; the science which he cultivated and the business which he pursued were so closely connected as to be almost identified. But how many branches of science are so intimately connected with any trade or profession? And even in this case, we venture to maintain that Hanbury might and would have done more for his speciality if he had held, for instance, an *emeritus* professorship. John Dalton maintained himself during a great part of his life by teaching the elements of mathematics. Surely no one can deny that every hour of his time thus occupied was an hour withdrawn from original physical and chemical research. If, indeed, scientific research is best carried on in the intervals of business, it is strangely unlike all other human pursuits. We hesitate, as a rule, to intrust anything of moment to the mere amateur.

We distrust the man who tries to combine even two mechanical trades of a nearly allied nature. We quote the old proverb about the "Jack of all trades and master of none." We admit that "no man can serve two masters." Yet we are some of us unreasonable enough to expect that biology, physics, chemistry may be successfully cultivated, or even *best* cultivated, in the scanty leisure of a man with a body more or less wearied and a mind more or less distracted with other thoughts, cares, and anxieties! Surely at the bottom of all this must lie the notion that science is after all a very paltry matter, not requiring for its successful pursuit any great amount of time, thought, attention, or energy. Yet at the same time, with an inconsistency perfectly refreshing to witness, we often suspect and distrust the professional man, or man of business, if we have reason to believe that he has a literary or scientific "hobby." We fear lest his researches should interfere with the due discharge of his business avocations. Whether his business may not rather interfere with the effective pursuit of his scientific investigation we never ask, such a matter being, we presume, unworthy our consideration. Says the work before us—"I know very well that some who could do admirable service to literature or science, and have accumulated abundant and valuable material, are restrained from making it of general use by publication from the knowledge of the fact that the public would not generally patronise a professional man who, as they say, has a hobby. To some extent, no doubt, this is a reasonable apprehension. I knew a case where a provincial medical man lost a valuable appointment which he had held for some years mainly because he had endeavoured to promote the study of geology in one of the finest geological districts in the whole country." With this quotation we must introduce the reader to an Essay on "Unencumbered Research: a Personal Experience," forming a portion of the work before us. In this paper Mr. H. C. Sorby ably, but very temperately, discusses the mistaken opinion which we are now examining in the light of his own private experience. We need scarcely say that when so eminent an authority as Mr. Sorby, who can look back on "nearly thirty years of almost uninterrupted practical scientific investigation," analyses, as it were, the processes by which scientific discoveries are made, and shows their general incompatibility with any distracting pursuit or avocation, he is entitled to a respectful hearing. Almost in all points we can endorse his views from our own experience. "For the successful prosecution of original enquiry," he declares, "two of the most essential requisites are abundance of time for continuous and extended experiments, and freedom from all those disturbing cares and engagements which either interrupt the experiments at critical times or so occupy the attention as to prevent the mind from properly digesting the results and deducing from them all the conclusions to which they should lead the investigator. Anyone who has had only a very small amount of experience in original research must have often been struck with the length of time necessary to arrive at a comparatively small result. Many experiments cannot possibly be made to go on in any other than their own slow way. Any attempt to hasten them would probably result in a complete loss of all the time spent over some previous part of the process, or of material which could not be replaced; and even with every care this loss cannot always be avoided."

But it is not merely the absolute amount of time which is the main point. There is here a most important distinction between the student of *books* and the student of *things*, and the difficulty falls upon the latter. The reader, suppose he has only a part of his time at his own disposal, can still leave his books ready, open if he prefers it, and can thus usefully fill up even portions of time of a few minutes duration. Not so the student of things. If he begins an experiment he must be able to attend to it at the precise moment required, or his operations will end merely in disaster. On returning he will find solutions evaporated to dryness, tubes or retorts cracked, temperatures that have exceeded the desired point. The unwatched experiment has led to absolutely nothing but loss of labour and material. Or say he is engaged in biological research—delicate tissues awaiting examination have entered into decomposition, phenomena to be observed have taken place in his absence and have not been recorded. Sometimes the golden moment to be snatched at may depend upon the weather; for the solution of some problem the student may require bright sun-light, or perhaps moon-light. He may wait for days, perhaps for weeks, for the desired condition. But if his time is not fully his own just at that moment, he may find himself fettered by some professional claim, and the opportunity may slip by.

But this is not all; every one who has been accustomed to watch the operations of his own mind must agree with Mr. Sorby that when grappling with any difficulty, theoretical or practical, the solution cannot be obtained at some given moment, as it were, to order. The man of science seeking the cause of some phenomenon as yet unaccounted for, and, we presume, in a manner strictly analogous; the physician pondering on the origin or the meaning of some strange symptom; the lawyer seeking to fill up some gap in a chain of argument, or the statesman considerating how some difficult negociation is to be carried to a successful issue—cannot sit down and say "Now I will solve the question." The ray of light which makes all plain and intelligible often darts into his mind in what are called idle moments. A casual word uttered in conversation, some trifling fact observed by the wayside and often apparently unconnected with the issue, seems to fire a train, and the mountain of difficulty that lies across our path is shivered to pieces in a moment. This point is ably illustrated by Mr. Sorby from incidents in his own career as an investigator:—"The more I studied the microscopical structure of these cleaved rocks (*i.e.*, those possessing slaty cleavage), the more I was puzzled with the observed facts. One day when quietly walking in my garden and reflecting on things in general, the simplest possible explanation of the whole flashed into my mind. I immediately went into my work-room, mixed some small pieces of coloured paper with wet pipe-clay, and on compressing them in the manner that slate rocks are proved to have been compressed, I found that I obtained a very good representation of the characteristic structure on which their cleavage depends. From that time forward the whole theory of cleavage took a new shape in my mind, and after studying by experiment with the microscope and in the field those facts which this mere hypothesis indicated as important, in a few years I had the satisfaction of observing that it was universally adopted as a perfectly satisfactory explanation of one of the great phenomena of geology."

To take another case—"In studying the structure of meteorites, the evidence of original igneous fusion becomes stronger and stronger, even in the case of the iron masses containing much olivine. This comparatively uniform distribution of such a heavy metal as iron, and of such a relatively light mineral as olivine, appeared the more to be incompatible with igneous fusion the more one became acquainted with what occurs in our furnaces. In the case of a melted mass of metallic iron and slag the separation is all but complete, and no such thing as an uniform mixture is to be found. For a long time this circumstance remained a puzzle; but in walking out in the country one summer evening, some trivial circumstance, long since forgotten, led me to perceive that the almost immediate separation of the metal and the slag was due to the powerful attraction of our earth, and that in any situation where the force of gravitation was very weak no such separation would occur. The general conclusion then became very simple. Meteoric masses of iron and olivine must have cooled from a state of fusion either as small masses in free space, or near the centre of large planets."

One more instance may be quoted, drawn from a different sphere of research:—"For many years, in common with all who had studied the subject, I had referred the position of the absorption-bands seen in the spectra of different colouring matters to an artificial scale, or, in a few instances, to the principal Fraünhofer lines. One day, however, whilst rambling over the quiet hills of Derbyshire it occurred to me, apparently quite accidentally, that such a system had no physical foundation, and that the time method was to express the position of all parts of the spectrum in terms of the wave-lengths of the light at each part. I at once set to work to contrive the means of doing this conveniently, and it was not long before I succeeded." "It would be easy to multiply such examples and to show the manner in which one train of ideas led to another. Very often the circumstances and train of ideas which led to a discovery were immediately forgotten in the face of the result, like the scaffolding used in the erection of some stately building, and sometimes the circumstances were connected with the main question in a manner so ridiculous that it would be altogether inappropriate to describe them."

From these cases we may surely infer that for the successful prosecution of research abundant and uninterrupted time is one of the main requisites; time not merely for actual thought, but leisure. It will at once strike the reader that in all the cases given by Mr. Sorby the happy thought occurred to him during gentle exercise and when the mind was perfectly at ease. This we can confirm, both from our individual experience and from the statements of friends. But what, then, is the case of a person who has no leisure, such as the man who endeavours at once to conduct a business or a profession, and to devote himself to scientific research? What if the ideas above mentioned had presented themselves to Mr. Sorby amidst the distractions of business? Might they not have been forgotten before an opportunity occurred of working them out and putting them to the test of experiment? Nay, might not their bearing have altogether escaped notice? One consideration our author appears to have overlooked which very much strengthens his—and our—position. It is this: not all the way-side suggestions which occur to our minds are really valuable. Multitudes must be dismissed as worthless. But everyone alike requires to be duly tested before its value can be known. This of course greatly augments the demands upon the time of the enquirer. But as Mr. Sorby remarks, "if the mind of an investigator is ready to take advantage of every circumstance that may occur, to press forward his enquiry in the line of truth, the removal of the most formidable difficulties is a mere question of time." In summing up the question he adds—in our opinion most truly—"One thing, however, is clear and cannot be denied; no one can even remotely approach to this mode of life and continuous observation whose mind is constantly engrossed with other cares—whose thoughts are necessarily directed to the consideration of how he can provide for the needs of each coming day, or how he can extricate himself from or avoid pecuniary embarrassment. Whatever the experience of others may lead them to think, mine has been amply sufficient to convince me that I never could have done what I have been able to do if it had been necessary for me to attend to any business or profession as a means of support. Though I wish I had been able to do more, yet if I had been interrupted by the cares of practical life I should certainly have done far less, and in all probability the general quality of the work would have been deteriorated to a still greater extent."

If, therefore, Mr. Sorby had not been in an independent position his contributions to science—and they are not small—would have been diminished or lost altogether. But how many men in easy circumstances possess the natural abilities and tastes, or have received the training, which incline and qualify them for a career of research? There is a proverb which tells us that "necessity is the mother of invention." In so far that when some particular public want has made itself felt and has attracted the attention of many minds, it will probably be met this adage is perfectly true. But if it be understood to mean, as we fear is sometimes the case, that personal want or distress may drive a man to scientific investigation who, if in better circumstances, would remain idle, we must with our whole soul protest against an absurdity so glaring. Men in distress naturally turn their attention to something immediately remunerative, which the most successful scientific investigation can rarely if ever be.

Another consideration here suggests itself. In the two last centuries there were in various parts of England rich coal beds lying near the surface and capable of being worked at a comparatively small outlay. Now, these easily accessible beds being exhausted the miner is compelled to sink deeper, and to work at a greater cost. Not otherwise is it in science. The facts and truths that lay near at hand have been already gathered in. We have now to go farther afield, to use costlier because rarer materials, to correct the approximate determinations of our predecessors, and in so doing to employ expensive instruments of precision. Little could be done in these days with apparatus such as that used by Dalton or by Davy in the outset of his career. Hence it has become more difficult for a poor man, unaided, to win his way to eminence.

With Mr. Sorby we admit that there are here and there exceptional positions in some of our manufacturing establishments where at any rate chemical and physical research may be carried on. But such opportunities are rare indeed. A "works' chemist" generally finds his whole time absorbed in routine duties, and is merely tantalised by opportunities of which he may not avail himself, phenomena which he is debarred from examining, and suggestions which he must suppress. We have known a "works' chemist" grossly insulted by the head of the establishment for having called his attention in private to an abnormal amount of arsenic in a sample of pyrites. "Determine nothing in future in pyrites but sulphur, copper, and silver" was the command. For sciences other than chemistry or physics manufactures afford little scope.

We must therefore pronounce it a delusion to expect that research can, to any worthy extent, be carried on "in the intervals of business," and must return to the conclusion that the nation which wants it and must have it must be prepared to endow it. But how?

Our first reply to this question brings us in immediate contest with the vexed topic of "university reform," and so lands us in a region of political and theological quarrels. When men cast about for funds for the endowment of research they remember naturally that the nett income of Oxford, taking the university and the colleges together, amounts to the startling sum of £400,000 a year. Of this income £91,545 a year are absorbed by the "Fellowships," which at Cambridge account for £92,820 annually. "These Fellowships at present are mere sinecures, with not even ostensible duties attached to their possession. The value of a Fellowship at each University may be taken to vary between £200 to £300 per annum, paid, for the most part, in the form of a life-pension, terminable on marriage. These prizes are awarded in open competition, either by special examination, or as an indirect result of a high place in the public examinations." Surely, then, these Fellowships are the very thing we are in search of for the endowment of research. But the moment this change is mooted, there rises from amongst those who think, or at least call, themselves Conservatives a hostile hum made up of the words "confiscation, spoliation, violation, communism, revolution, confusion, pious founders," and the like; and amidst the sea of sound we catch exhortations to this effect:—"Our Universities are educational or tuitional, not investigational. If you want places for the advance and increase of human knowledge, found and endow them yourselves!"

At the same time Liberalism, so-called, stands chiefly mute, fearing lest its Chinese idol, competitive examination, should be overthrown, and his joss-house burnt in the struggle. A few only are found who declare boldly that, *per fas aut nefas*, the dead hand of the pious founder notwithstanding, such sums must be seized and applied to a more useful purpose. But what if both parties, on closer examination, are found to be in the wrong? What if the skeleton-hand of the pious founder, instead of being held up in menace to bar our way, is found to be beckoning us on? What if we require to effect the needed reforms, no confiscation, but merely a carrying out of the original design of old worthies? Contrary to the belief alike of their friends and their foes, our great national universities are not exclusively and intentionally mere places of education. It was never the intention of the founders, whether of the universities or of their individual colleges, to make them mere places where a set of young men were to be crammed for examination. The advancement of knowledge was an end constantly kept in view. The Fellows were supposed to be a body of men giving up the whole of their time to study and research. Thus, in Merton College, the Fellows "are to employ themselves in the study of Arts, of Philosophy, the Canons, or

Theology, but the majority are to continue in the liberal Arts and Philosophy." In the statutes of New College, founded by William of Wykeham, under charter from Richard II., the following passage occurs :—

"We desire, moreover, that our scholars (the original name of the Fellows) occupied in diverse sciences and faculties may, by their intercourse with each other, learn something new every day, and by continual advance become better and better, that the spirit of the whole multitude tending to the same end may be one, and that through the Divine mercy our colleges endowed with and supported by men of so many sciences may the more firmly and securely abide and continue for ever in the beauty of peace."

In All Souls College there were to be forty Scholars or Fellows, who were to study without intermission. Of these twenty-four were to study the Arts and Philosophy, or Theology.

In New College it is expressly stipulated that two of the Fellows might devote themselves to Astronomy, and two to Medicine.

In Magdalen College, of the seventy "poor and indigent Scholars" forty, called "Fellows," were to study Theology and Moral and Natural Philosophy.

Many more such extracts from the statutes of the colleges might be given, all tending to prove that, in the words of the Report,* "The duty of the Fellows, as such, was, as we shall show more at length hereafter, *not to teach, but to learn;* hence the earliest name of this class . . . . . scholares. (P. 134.)

"The most important object of colleges was, as Blackstone states, '*ad studendum.*' To receive and not to give instruction was the business of the Fellows of colleges. The founder of Queen's has expressly declared that he intends by his benefaction to relieve his fellows from the necessity of teaching. In all colleges, even in those which aimed at supplying instruction to the university, the great majority of the Fellows were intended to devote their life to study, and not to engage in teaching, either in the college or in the university."†

Summing up the evidence thus collected from the statutes themselves and from the Commissioners' Report, the essayist declares that the "*fellowship system originated in a desire to promote study and not to promote teaching.*" Their purpose was not educational, tuitional, but investigational. "It is certain that the present generation of Fellows, whether tutors or non-residents, are alike wrongful occupants of their comfortable positions, and usurpers when tried at the bar of history."

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* First University Commission of 1852.

† Report, p. 140.

Claiming, then, as we do, these positions for men capable of and devoted to research, we are assuredly not revolutionists, spoliators, and confiscators, but upholders of the rights of property and of the sacredness of bequests. Old precedent speaks not against us but for us. What we urge is not an innovation. "Up to the close of the last century Oxford and Cambridge always took the lead of the nation in all intellectual matters. Their pre-eminence was due not to the efficiency of their instruction, but to the presence of the few industrious holders of sinecure appointments. . . . . It is more to the purpose to draw attention to the neglected fact that even in the physical sciences have college fellowships proved themselves in the past to be not unkindly nurses.

"It can hardly have escaped the attention of the reader that in the extracts from the statutes given above, Medicine, Natural Philosophy, and Astronomy hold a considerable place. It is indeed only a deplorable ignorance of history which induces people to regard the present revival of scientific studies at the Universities as an absolute innovation, characteristic of the latter part of the nineteenth century. William of Wykeham contemplated that two of his Fellows should apply themselves to Medicine and two to Astronomy; while William of Waynflete, his worthy imitator, named Natural Philosophy as one of the three chief objects of study at Magdalen. Nor has the actual fruit been altogether unworthy of these liberal designs. It was Thomas Linacre, a Fellow of All Souls, a man who combined to a rare degree classical taste with scientific erudition, that first raised the practice of medicine to an honourable status in this country, and induced Cardinal Wolsey to found the Royal College of Physicians in London at the commencement of the sixteenth century. Of that learned body Linacre was the first President. He was succeeded in the chair by Dr. Caius, himself a Fellow of Gonville Hall at Cambridge, which he subsequently erected into the College which bears his joint name. In later days Sydenham, the father of the scientific study of medicine, was a student of Christ Church, and testified to his intimate connection with that corporation by bequeathing to it his valuable library. It ought, also, never to be forgotten that it was among a select band of Oxford Fellows and their friends that the Royal Society first saw the light. In Astronomy, besides the peerless name of Newton, who was emphatically an academical recluse, the lists of eminent professors who were not teachers, both at Oxford and Cambridge, might be quoted, of whom Bradley is only one among many. These examples have not been discovered by means of any remote enquiry, but are merely jotted down as they suggest themselves to one who has no books of reference at hand. Their number might be indefinitely extended, but as it is they are more than sufficient to demonstrate that in every branch of knowledge Oxford and Cambridge have fairly held their own; and that *endowed sinecures did not turn out a failure till it was announced, with quasi-Parliamentary* sanction, *that the traditional duty of study was no longer to be expected from the Fellows.*"

We have merely, then, to restore the universities to something of their old condition, though on the wider and deeper scale suitable to the modern position of science and learning. We must cast aside, on the one hand, the corruptions and neglect of the eighteenth century, and on the other the pseudo-reforms of the nineteenth. All this can be done by the enactment of a new principle in the attainment of fellowships, and indeed of all high honours; instead of election with its jobbery and favouritism and corruption, or competitive examination with its cram and quackery and superficiality, and hostility to all true intellectual life, let us have research as the only test. One of the writers of the work before us gives it as his opinion—"Only when a man has finished his training and proved his competence by some published investigation should he be appointed to a fellowship." Although speaking of theological research he thus advocates our scheme, since the principle is one and the same. We are far from claiming the whole of the fellowships for physical science. We are pleading the cause of investigation in philology, in history, in theology, as well as in biology, chemistry, physics, astronomy, or geology. Let there be a fair division. Then let every candidate for a fellowship be bound to produce some piece of original research in the science or branch of erudition to which he intends to devote his life. Of course only students who have duly passed through their proper curriculum, and who have proved that they have reached a certain preliminary standard, will be allowed to submit to this new trial. It will, however, be much less of a competition than the present system, since a great number of minds—those, indeed, of the class who now often make the greatest figure—will find themselves at once disqualified. The successful candidate will then hold his life-pension, *on condition of work*, a minimum scale being probably decided on at the outset. But as no one can on this system possibly attain a fellowship who has not proved himself able and willing to enter upon original research, there will be, we think, little fear of idleness. As for the unsuccessful candidates—those whose attempts at original research are found inadequate—they will not, like the defeated, or, in fact, like the successful candidates in our competitive examinations, be useless creatures. The training they have received will make them very valuable in certain positions. If they have selected dynamics, physics, or chemistry, an honourable career in the arts and manufactures may still be open to them. But this one alteration will pass through the entire university, and operate a beneficial change in every department. The students will flock to those professors who will be best qualified to initiate them into the mysteries of actual research, and Oxford and Cambridge will become once more centres of earnest progressive scientific activity, regularly pouring forth their streams of discovery. Such establishments as Owens College, the Yorkshire College of Science, those of Birmingham, Bristol, and Newcastle, all which we hope soon to congratulate as formally constituted universities, will doubtless, in course of time, have their fellowships to offer on similar conditions. It is an encouraging fact that wealthy and benevolent individuals are now evidently more disposed to give or to bequeath large sums for the encouragement of higher education,—surely a more useful employment of their funds than the old custom of founding charity-schools, where some score or two of unfortunate urchins were dressed up in absurd costumes, to be the sport and the butt of the neighbourhood. A few fellowships, open only to the authors of the best piece of original research, say in biology or chemistry, would be invaluable appendages to these our new schools of science. Even where no fellowships, *emeritus* professorships, or other endowments exist, research, if not actually subsidised or endowed, is easily capable of promotion. In conferring degrees the production of some original investigation might be advantageously demanded from the candidate, either in addition to the present examinations, or, perhaps still better, in lieu thereof. Let us, for instance, take the degree of Doctor of Science, as conferred by the University of London. Suppose that a Bachelor of Science, instead of submitting to the usual examination, had the option of earning the higher step by the producing of an original paper in connection with any branch of science in the course. It would be a healthy change if, instead of speaking of a student "reading" for this or the other degree, we could ever come to speak of his "working" for it.

We have spoken of the system of competitive examinations as a Chinese invention; but it has also another root. As one of the essayists before us observes—"The introduction of the system of emulation into the higher studies is historically traceable to the Jesuits. The adoption of the principle of perpetual supervision of repeated examinations, of weekly exercises, produced marvellous effects in the Jesuit colleges. It was not till the first half of the eighteenth century that opinion began to turn. It was then found that beneath this brilliant show of college exercises and prizes was concealed a starved and shrivelled understanding. The work done in class was pattern-work, but the pupil whom the institution turned out was a washed-out, frivolous, superficial being." Whilst fully recognising that such must be the result in every seat of learning where examinations are made a leading feature,* we believe that the Jesuits introduced into their system one arrangement in which English colleges are exceptionally deficient. They recognised the extreme diversity of individual tastes and abilities, and the no less indisputable truth that there are many distinct methods for effecting the discipline of the intellect. They consequently did not force every pupil through one undeviating course, but gave him his choice between equivalent studies.

In this country science has been hardly dealt with, both by its friends and its enemies. The former have urged its claims rather on the score of utilitarian results than as a means of mental training; and the latter, without a particle of evidence, have assumed its inferiority. In Germany specialism begins at the very gate of the university, and the student selects at once his faculty. That this arrangement facilitates profound scientific research we feel certain.

In the following passage Prof. Max Müller, substantially and most ably, pleads for the kind of university reform which we have been proposing:—"Whatever fellowships were intended to be, they were never intended to be mere sinecures, as most of them are at present. It is a national blessing that the two ancient universities of England should have saved such large funds from the shipwreck that swallowed up the corporate funds of the Continental universities; but, in order to secure their safety for the future, it is absolutely necessary that these funds should be utilised again for the advancement of learning. Why should not a fellowship be made into a career for life, beginning with little, but rising like the incomes of other professions? Why should the grotesque condition of celibacy be imposed upon a fellowship instead of the really salutary condition of no work no pay? Why should not some special literary or scientific work be assigned to each Fellow, whether resident at Oxford or sent abroad on scientific missions? Why, instead of having fifty young men scattered about in England, should we not have ten of the best workers in every branch of human knowledge resident at Oxford, whether as teachers, guides, or examples? All this might be done to-morrow without any injury to anybody, and with every chance of producing results of the greatest value to the universities, the country, and to the world at large."*

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* We have known a college where examinations were held at quite irregular intervals, without even an hour's previous warning. This stratagem rendered cram, and indeed every form of special preparation, impossible, and gave the heads of the establishment a far clearer insight into the attainments and powers of their students than could be obtained by any other form of examination.

Turning from university reform considered in the interests of "research," we have yet to speak of certain establishments, national and public, in which the services of scientific men are required. There are museums, observatories, botanical gardens; there are geological surveys and occasionally exploring expeditions. In all these there are positions which afford scope, greater or less, for research. But how are they filled? Formerly, we believe, candidates were selected by interest. Now competitive examination reigns. Would it not be better, except in case of men who have already "won their spurs," to adopt our test, and thus secure the right man in the right place?

But we cannot stop here; even when we have converted our universities, present or nascent, into centres of investigation and made their highest honours and their more tangible rewards accessible solely by the way of successful research, something more is wanting. It does not suit the national mind to be tied down too rigidly. We must make provision, therefore, for the encouragement of research among those who may have aimed at a college fellowship, but whose performances have not been found of sufficient value; among such as, though educated at one of the universities, have not found it consistent with their plans and duties to offer themselves as candidates for fellowships or professorships; among British subjects educated abroad, and, lastly, upon the important class who have qualified themselves for research by private study without having ever been connected with any college or public institution. Nay, even those who have won a fellowship and hold it, according to our proposal, on condition of not remaining idle, would work all the more zealously if in prospect of further rewards consequent upon success.

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* Chips from a German Workshop, iv., 4—10.

We suggest, therefore, that some learned body or bodies of high position should have the duty of examining all papers sent in and should be enabled to reward the authors of such as are found of value with a round sum of money proportionate to their merits. The details of such a scheme will of course require long and careful consideration, but we submit that the most suitable body to be entrusted with a task so responsible and so honourable would be the Royal Society. From its midst it would be easy to select a committee fully competent to pronounce on the value of any paper placed in their hands and no less above suspicion of foul play. As to the requisite funds, if Henry Cavendish had only carried out his original plan of bequeathing his large portion to the Royal Society for the furtherance of research no difficulty would have existed. As, however, he unfortunately altered his intention, the needful resources would have to be provided either by a Parliamentary vote or by private bequests, donations, and subscriptions. Those who mistrust the impartiality of the Royal Society are reminded that the application of funds obtained in either of these ways would be jealously scrutinised, and that any suspicion, even of favouritism, would at once lead to a rigid investigation. The papers sent in might, as in the case of tenders for a contract, be signed, not with the author's name, but with some motto or cypher. As to the scale of reward, we should suggest that a paper pronounced worthy of insertion in the "Philosophical Transactions," should entitle its author to—say £500.

As an alternative proposal, the task of considering and rewarding original investigation, instead of being committed exclusively to one body, might be distributed among several, as, *e.g.*, the British Association, which does already assist research by sums voted as subsidies to men of science engaged with certain specified questions. The Astronomical, Linnean, Geological, and other Societies, might also be proposed as qualified and entitled to a share of this duty. To us, however, there appear weighty objections to any division.

There might occur conflicts of jurisdiction, and widely different estimates of the value of work done. Two papers substantially identical might be sent, by one and the same author, to two different Societies. A dissertation rejected by one authority might be entertained by another, to the occasioning of no little scandal. Subdivision could, moreover, hardly fail to weaken the sense of responsibility. As, moreover, the Royal Society rarely fails to include all the most eminent men in every department of science, it cannot be urged that any paper could find in any other Society a more competent body of judges.

In submitting these schemes for the promotion of research in England, or rather amongst the English nation, we are far from conceiving them as incapable of improvement. All we desire is that our country shall not fall short of any foreign land in the value and the extent of its contributions to science; and if this end can be more effectually promoted in any other manner than in those we have suggested, we shall be the first to abandon our own views and to labour for the adoption of the more excellent way.

Mr. Dyer says—" Everywhere the harvest of new knowledge is maturing and ripening. England alone is loth to send labourers to reap it."

V. THE BOOK OF THE BALANCE OF WISDOM.

By H. Carrington Bolton, Ph.D.

So rapid are the strides made by science in this progressive age, and so boundless is its range, that those who view its career from without find great difficulty in following its diverse and intricate pathways, while those who have secured a footing within the mysterious domain and are free to journey on the same road are often quite unable to keep pace with its fleet movements and would fain retire from the unequal contest. It is not surprising, then, that those actually contributing to the advancement of science, pressing eagerly upward and onward, should neglect to look back upon the labours of those who precede them, and should sometimes lose sight of the obligations which science owes to forgotten generations.

Could the wisdom of the world concealed in the silent unwritten history of past ages be divulged by a miracle of revelation, how startling and interesting would be the wonderful disclosure! Imagination fails to conceive of the possible social and scientific status of the present era, had the named and the unnamed "lost arts" been preserved through all time, and had the experience of the human race been uninterruptedly handed down to the existing generation.

The legitimate feeling of exultation and satisfaction enjoyed by oriental scholars when years of painstaking research amid the musty records of the past are at length rewarded by a literary and historical discovery of importance, seems to us comparable to the pleasurable excitement experienced by the scientists whose investigations of nature are crowned by the determination of a new species or the establishment of a new law. In this respect, the Egyptologist and the Naturalist, the student of ancient history and the student of modern chemistry, have a common purpose, each in his own sphere seeking the truth.

Original investigations in the field of history are, however, vouchsafed only to those profound linguists whose erudition and skill in deciphering semi-obliterated cryptographs have been the fruit of a lifetime's laborious study; and these, absorbed in their study of the ancient, too often neglect to compare the wisdom of early times with the progress of modern scientific truths, and fail to appreciate the points most valuable to the student of science. Hence the history of science yet remains to be written.

This suggestion may be met with references to the works of the *savants* who, especially in the preceding century, devoted much to the elaboration of historical treatises in their several departments of science; but these are few in number, and, as we believe, merely sketch the outlines, the details of which will yet be supplied by some mighty genius at once a linguist, an archæologist, and a scientist.

Meanwhile, in default of the erudition which alone allows of critical examination of monumental inscriptions, papyri, and original manuscripts, the humble searcher after knowledge must be content to study available translations, notes, and criticisms provided by oriental scholars, and to bring into prominence such materials as his imperfect powers can command.

Bearing in mind the great obligations which the exact sciences owe to the Arabian philosophers of the Middle Ages, it is not surprising that such of their works as are made available by the translations of linguists afford abundant and rich sources of information to the student of the History of Science. The work named at the beginning of this article forms a remarkable contribution to the early history of the determination of specific gravities. This "Book of the Balance of Wisdom" is an Arabic treatise on the water-balance, written in the twelfth century, for an account of which the historian of science is indebted to the Chevalier N. Khanikoff, some time Russian Consul-General at Tabriz, an important city of Northern Persia. M. Khanikoff having obtained access, in some manner not explained, to a manuscript copy of the Arabic work, translated into the French language copious extracts, and prepared an analysis of its contents: these data, together with a transcript of the original Arabic version, he communicated to the American Oriental Society. The Society's Committee of Publication, in preparing the Russian Consul's work for their Journal, translated his notes into English, re-translated the Arabic extracts, and added their own valuable comments. The completed article is found in the sixth volume of the "Journal of the American Oriental Society," pp. 1 to 128, published in 1859.

The "Book of the Balance of Wisdom" treats exclusively of the balance, and of the results obtainable by this instrument, which has given to modern science so many beautiful discoveries. Its exposition of the principles of the centres of gravity, of researches into the specific gravity of metals, precious stones, and liquids, shows these Orientals to have attained to experimentation, a step in the progressive knowledge of physical truths entirely unknown to the ancients. Before offering such citations from this work as may seem necessary to establish this proposition, we will endeavour to answer two questions which naturally suggest themselves to our readers. What is the date of the original manuscript, and who was its author? Most happily both queries admit of satisfactory replies, based on internal evidence.

The dedication of the work proves it to have been composed at the Court of the Saljuke Sultan Sanjar, who reigned over a large part of the ancient Caliphate of Bagdad from A.D. 1117 to 1157. In this introduction the author appeals to this potentate in the following fulsome expressions of homage characteristic of the Orientals:—

"Most magnificent Sultan, the exalted Shah of Shahs, the king of subject nations, the chief of the Sultans of the world, the Sultan of God's earth . . . the shelter of Islamism and of Muslims, the arm of victorious power . . . Prince of Believers—may God perpetuate his reign and double his power! For his felicity is the illuminating sun of the world, and his justice its vivifying breath."

And immediately following this passage, occurs mention of the date:

"I sought assistance from his beams of light irradiating all quarters of the world, and was thereby guided to the extent of my power of accomplishment in this work, and composed a Book on the Balance of Wisdom for his high treasury, during the months of the year 515 of the Hegira of our Elect Prophet Mohammed—may the benedictions of God rest upon him and his family, and may he have peace!"

This proves the treatise to have been written in the years 1121-1122 A.D. At this period of the world's history we find Arab philosophers cultivating literature and science, while the rest of Europe was just immerging from intellectual darkness. The religious world had scarce recovered from the intense excitement aroused by Peter the Hermit and his followers, who led the tumultuous rabble 600,000 strong towards the Holy City; and priests, knights, and peasants were preparing with frantic zeal for a Second Crusade. Abélard and Héloise were names which afforded endless discussions in the cloister and on the hearth. Science was at a low ebb, a century elapsed before Albertus Magnus and Roger Bacon exerted their influence; and scholastic philosophy, attaining its loftiest height, swayed the intellects of the age.

The authorship of the Book of the Balance of Wisdom is easily determined by the fortunate circumstance that the author names himself several times, "but in so modest a manner as scarcely to attract attention; instead of heralding himself at once, in his first words, after the usual expressions of religious faith, as Arab authors are wont to do, he begins his treatise by discoursing on the general idea of the balance" and then simply remarks: "*Says al-Khazini, after speaking of the balance in general,*" and proceeds to enumerate the advantages of the balance which he is about to describe. Two other passages in the extracts furnished by M. Khanikoff satisfy the Oriental scholars who have examined them that the author is the self-named al-Khazini.

Attempts to identify al-Khazini with individuals of historical fame have given rise to differences of opinion, but the weight of evidence is in favour of regarding him as the same with Alhazen, the Arab optician and physiologist.

Alhazen seems to have been a native of Persia, and to have resided in Spain and Egypt, but of his biography little is known. He is especially distinguished for his demonstration of the theory of vision, showing that the rays of light are reflected from external objects to the eye, and do not issue forth from the eye to impinge on external things, as up to his time had been taught.

This explanation, moreover, was not based on mere hypothesis, but was the result of anatomical investigations as well as of mathematical discussions.

Alhazen also explained the astronomical refraction of light, its dependence on the variation of the density of the media traversed, and its influence in producing the phenomenon of twilight. In the discussion of all these problems he evinced true scientific greatness. He favoured the theory of the progressive development of animal forms, anticipating a doctrine but newly obtaining acceptance. Dr. J. W. Draper,* who has been our guide in this connection, says of Alhazen: "Though more than seven centuries part him from our times, the physiologists of this age may accept him as their compeer.

The name al-Khazini signifies "related to the treasurer," which accords with his statement that the work was composed for the royal treasury.

The Book of the Balance of Wisdom begins with a dedication to God "the compassionate, the merciful," and a pious statement of the author's religious faith. An introduction, divided into eight sections, then follows; in the first section, the advantages and uses of the Balance are enumerated in this language:

"These advantages are:—1, exactness in weighing; this balance shows variations to the extent of a mithkal,† or of a grain, although the entire weight is a thousand mithkals, provided the maker has a delicate hand, attends to the minute details of the mechanism, and understands it; 2, that it distinguishes pure metal from its counterfeit, each being recognised by itself without any refining; 3, that it leads to a knowledge of the constituents of a metallic body without separation one from another . . . . . .; 4, that it shows the superiority in weight of one of two metals over the other in water, when their weight in air is the same, and reversely . . . . . .; 5, that it makes the substance of the thing weighed to be known by its weight . . . .; 6, ...; 7, the gain above all others——that it enables one to know what is a genuine precious stone, such as a hyacinth, or ruby, or emerald, or a fine pearl; for it truly discriminates between these and their imitations or similitudes in colour, made to deceive."

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* Hist. Int. Devel. Europe, page 360.

† We connot here undertake to discuss the ancient Arabic system of weights, but merely state that while authorities differ, M. Khanikoff, after a careful examination and comparison of modern and ancient standards of weights in Georgia, Daghistan, and elsewhere, where Arabic customs have suffered little change, assigns to the mithkal the value of 4·5 grammes. The mithkal, according to Abu-r-Raihan (quoted below) equals 6 daniks; 1 danik = 4 tassujs; 1 tassuj = 4 grains; and 1 grain = 4 barley-corns.

Then follows the theory of the water balance; and in the fourth section some account of its early history and the well-known narrative of King Hiero's crown.

"It is said that the [Greek] philosophers were first led to think of setting up this balance, and moved thereto, by the book of Menelaus* addressed to Domitian, in which he says: O King, there was brought one day to Hiero, King of Sicily, a crown of great price, presented to him on the part of several provinces, which was strongly made and of solid workmanship. Now it occurred to Hiero that this crown was not of pure gold, but alloyed with some silver; so he inquired into the matter of the crown, and clearly made out that it was composed of gold and silver together. He therefore wished to ascertain the proportion of each metal contained in it, while at the same time, he was averse to breaking the crown on account of its solid workmanship. So he questioned the geometricians and mechanicians on the subject, but no one sufficiently skilled was found among them, except Archimedes the geometrician, one of the courtiers of Hiero. Accordingly he devised a piece of mechanism which, by delicate contrivance, enabled him to inform King Hiero how much gold and how much silver was in the crown, while yet it retained its form. This was before the time of Alexander. Afterwards Menelaus [himself] thought about the water-balance and brought out certain universal arithmetical methods to be applied to it; and there exists a treatise by him on the subject. It was then four hundred years after Alexander."

Al-Khazini takes pains in this sketch of the early history of specific gravity, to establish dates by reference to contemporaneous individuals, but his chronology is evidently at fault. The Hiero alluded to is Hiero II., who died 216 B.C., while Alexander the Great lived more than a century earlier (356 B.C.—323 B.C.)

This Arabic version of the anecdote of Hiero's crown lacks the piquancy and interest of the narrative as originally given by Vitruvius, and is moreover not an accurate transcription; the words "devised a piece of mechanism" convey the impression that Archimedes constructed some peculiar form of apparatus with which to solve the problem. Indeed this story, familiar as it is, is not unfrequently erroneously related, even as al-Khazini himself has done, and compilers of text-books of natural philosophy, content to copy from each other instead of seeking the original sources, transmit the errors of detail.

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* A celebrated mathematician who lived in the reign of Trojan, 98—117 A.D., and author of a treatise on Spherical Geometry.

These reflections are in part suggested by a singular construction given to the narrative in a recent and really excellent text-book on the "History of Natural Science," in which the authoress commits the remarkable anachronism of representing (in a woodcut) the crown and the metallic masses suspended in water from *spring-balances* of modern appearance and construction.

To dispel any lingering ideas of this character we here revive the passage in Vitruvius,* "*De Architectura,*" the original source of the narrative, and in which it appears that the "greatest geometer of antiquity" arrived at his results by a comparison of unequal volumes of water obtained by displacement, and not by direct weighings in that liquid.

"BOOK IX. CHAPTER 3. *Of the method of detecting silver when mixed with gold.*"

"Though Archimedes discovered many curious matters which evince great intelligence, that which I am about to mention is the most extraordinary. Hiero, when he obtained the royal power in Syracuse, having, on the fortunate turn of his affairs, decreed a votive crown of gold to be placed in a certain temple to the immortal gods, commanded it to be made of great value, and assigned an appropriate weight of gold to the manufacturer. He, in due time, presented the work to the King, beautifully wrought, and the weight appeared to correspond with that of the gold which had been assigned for it. But a report having been circulated that some of the gold had been abstracted, and that the deficiency thus caused had been supplied with silver, Hiero was indignant at the fraud, and unacquainted with the method by which the theft might be detected, requested Archimedes would undertake to give it his attention.

"Charged with this commission, he, by chance, went to a bath, and being in the vessel, perceived that as his body became immersed, the water ran out of the vessel. Whence, catching at the method to be adopted for the solution of the proposition, he immediately followed it up, leaped out of the vessel in joy, and returning home naked, cried out with a loud voice that he had found that of which he was in search, for, he continued exclaiming in Greek, Ευρηκα (I have found it out).

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* Marcus Vitruvius Pollio, a distinguished Roman architect and author, served as military engineer under Julius Cæsar in Africa, B.C. 46. His work, "De Architectura" (written in his old age), comprising ten books, is the only ancient treatise on the subject extant. Of Vitruvius's biography very little is known.

"After this he is said to have taken two masses, each of a weight equal to that of the crown, one of them of gold and the other of silver. Having prepared them he filled a large vase with water up to the brim, wherein he placed the mass of silver, which caused as much water to run out as was equal to the bulk thereof. The mass being then taken out, he poured in by measure as much water as was required to fill the vase once more to the brim. By these means he found what quantity of water was equal to a certain weight of silver. He then placed the mass of gold in the vessel, and on taking it out, found that the water which ran over was lessened, because the magnitude of the gold mass was smaller than that containing the same weight of silver.

"After again filling the vase by measure, he put the crown itself in, and discovered that more water ran over than with the mass of gold, that was equal to it in weight; and thus from the superfluous quantity of water carried over the brim by the immersion of the crown, more than that displaced by the mass, he found by calculation, the quantity of silver mixed with the gold and made manifest the fraud of the manufacturer." *

Continuing the sketch of the history of the water-balance given by al-Khazini in the fourth section of the introduction, we find references to several Arabian philosophers, among them the celebrated physician Avicenna (Ibn Sina) who "distinguished [the components of] a compound scientifically and exactly," and Abu-r-Raihan "who took observations on the relations of [different] metallic bodies and precious stones, one to another, as indicated by this balance."

Al-Khazini also states that the instrument in question was called "the Physical Balance" by Mohammed Bin Zakariya of Rai, and it was named "the Balance of Wisdom," by the "eminent teacher Abu-Hatim al-Muzaffar Bin Ismael of Isfazar."

Abu-r-Raihan alluded to above is often quoted by al-Khazini and deserves our attention. He was a distinguished Arabian astronomer, born about 970, and died 1038 A.D. He was a member of the Society of Savans founded in the capital of Kharizm, and of which the eminent physician Avicenna

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* The Architecture of Marcus Vitruvius Pollio, in ten books. Translated from the Latin by Joseph Gwilt, London, 1826, pp. 264, 265.

was a shining light. He was the author of a number of works on astronomy, cosmography, and physics, one of which entitled "The Book of the Best Things for the Knowledge of Mineral Substances," and contained in the Ayin-Akbari, or Institutes of the Emperor Akbar, treats of the specific gravity of bodies and of hydrostatic methods for determining them. It is this work to which al-Khazini refers. A review of Abu-r-Raihan's treatise has been published by Mr. J. J. Clément-Mullet, under the title, "Recherches sur l'Histoire Naturelle et la Physique chez les Arabes." * Since this Arabic manuscript is probably the most ancient work extant which systematically treats of specific gravities, we make another digression and give a brief synopsis of its contents.

It contains theoretical explanations of the origin and formation of mineral bodies founded on the views of the Greeks, and particularly on those of Aristotle. According to these views, the variety of the weights of bodies depends upon the dry and the moist exhalation from them; air and water are the elements of these vapours, air giving the lightness and water the heaviness. The author then proceeds to describe methods for determining the specific gravity of bodies in the following words:—

"Scientific men determine by means of water the measure of these differences in weights. They prepare a vessel filled with water in which they introduce 100 mithkals of each of the metals; the quantity of water thrown out by each gives the difference in volume and weight, that one which displaces the largest bulk of water has consequently the largest volume but the least density, and that one which displaces the least water is the heaviest."

Abu-r-Raihan gives in tabular form the specific gravity of nineteen substances, nine of which are minerals and nine are stones. In the following table we annex the values assigned by modern authorities, showing the remarkable accuracy, in most instances, of the early determinations:—

TABLE OF SPECIFIC GRAVITIES.

	Abu-r-Raihan.	Modern Authorities.
Gold	19·05	19·30
Mercury	13·58	13·568
Lead	11·33	11·346
Silver	10·35	10·52
Bronze	8·82	8·05 to 8·95

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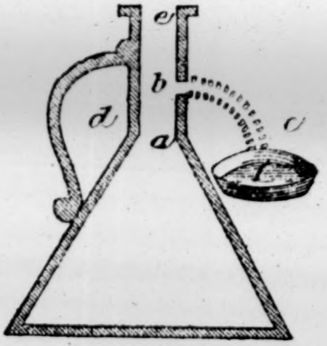
* Journal Asiatique. Serie v., vol. xi., p. 379.

	Abu-r-Raihan.	Modern Authorities.
Copper	8·70	8·78
Brass	8·57	8·58
Iron	7·74	7·79
Tin	7·31	7·29
Sapphire.	3·97	3·99
Oriental Ruby	3·58	3·90
Ruby	3·85	3·52
Emerald	2·75	2·73
Pearl	2·69	2·75
Lapis Lazuli	2·60	2·90
Cornelian	2·56	2·61
Amber (?)	2·53	1·08 (?)
Rock crystal	2·50	2·58

In the third lecture of the "Book of the Balance of Wisdom" al-Khazini describes a form of specific gravity flask which he calls the "conical instrument of Abu-r-Raihan" and to whom he apparently ascribes the invention. A mere inspection of the accompanying cut, a fac-simile of that in the original manuscript, together with the explanations (also from the original), suffices to show its nature and the method of using it. The author remarks that "the instrument is very difficult to manage, since very often the water remains suspended in the lateral tube, dropping from it little by little into the scale of the balance." This passage shows that Abu-r-Raihan had noticed capillary attraction; it is also certain that he understood that the size of the neck of the instrument affected the delicacy of the determinations, for he says he would have it "made narrower than the little finger but for the difficulty of removing through a smaller tube the bodies immersed in the water."

FIG. 1.



a. Neck of the instrument. *b*. Perforation. *c*. Tube in the form of a water-pipe. *d*. Handle of the instrument. *e*. Mouth of the instrument. *f*. Place of the pan (of the balance).

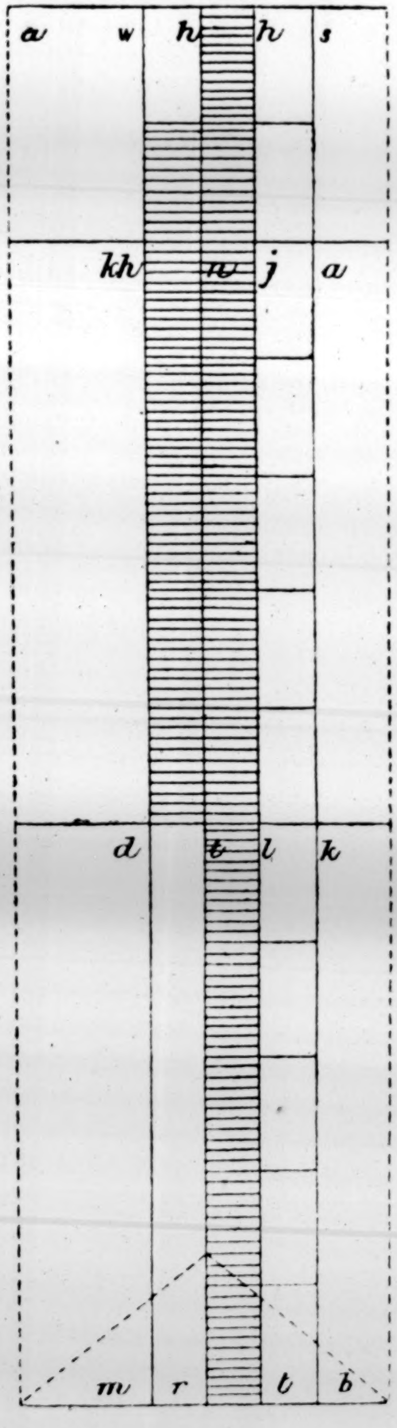
Al-Khazini's work is made up of eight lectures, each lecture includes several chapters and each chapter has several sections; to give the table of contents entire would undesirably lengthen this article, and we prefer to quote Al-Khazini's own summary of his treatise as contained in the sixth section of the introduction.

"I have divided the book into three parts:—I. General and fundamental topics, such as heaviness and lightness; centres of gravity; the proportion of the submergence of ships in water; diversity of the causes of weight; mechanism of the balance and the steelyard; mode of weighing with it in air and in liquids; the instrument for measuring liquids, in order to ascertain which is the lighter and which is the heavier of the two, without resort to counterpoises; knowledge of the relations between different metals and precious stones in respect to [given] volume; sayings of ancient and modern philosophers with regard to the water balance and their intimations on the subject. This part includes four lectures of the book in their order. II. Mechanism of the balance of wisdom; trial of it; fixing upon it of [the points indicating] the specific gravities of metals and precious stones; adoption of counterpoises suited to it; application of it to the verification of metals and distinguishing of one from another [in a compound], without melting or refining, in a manner applicable to all balances; recognition of precious stones and distinction of the genuine from their imitations or similitudes in colour. There are here added chapters on exchange and the mint, in connection with the mode of proceeding, in general, as to things saleable and legal tenders. This part embraces three lectures. III. Novelties and elegant contrivances in the way of balances such as: the balance for weighing dirhams and dinars without resort to counterpoises; the balance for levelling the earth to the plane of the horizon; the balance known as the 'even balance' which weighs from a grain to a thousand dirhams or dinars by means of three pomegranate counterpoises; and the hour-balance, which makes known the passing hours, whether of the night or of the day, and their fractions in minutes and seconds, and the exact correspondence therewith of the ascendant star and fractions of a degree. This part is in one lecture."

In the seventh chapter, which treats of the "Mechanism of the Instrument for measuring Liquids . . . and Application of it according to the philosopher Pappus the Greek," we find a description of a hydrometer.

"The length of this instrument, which is cylindrical in

FIG. 2.



shape, measures half a hand-cubit; and the breadth is equal to that of two fingers, or less. It is made of brass, is hollow, not solid, and the lighter particles of brass are carefully turned off by the lathe. It has two bases, at its two ends, resembling two light drum-skins, each fitted to the end, carefully, with the most exact workmanship; and on the inner plane of one of the two bases is a piece of tin, carefully fitted to that plane by the lathe, shaped like a funnel, the base of which is the drum-skin itself. The instrument being thus made, when put into liquid in a reservoir or vessel, it stands upon it in an erect position and does not incline any way."

The author then describes at length the manner of graduating the instrument, the decimal system being employed throughout. He remarks that the weight of the funnel-shaped piece of tin must be varied according to the density of the water assumed as a standard. Tables of the specific gravities corresponding to the marks on the instrument accompany the detailed account of its application.

The annexed figure of the hydrometer of Pappus (Fig. 2) does not give a very clear idea of the instrument, and is intended to show chiefly the manner of constructing and graduating it. The details given in the manuscript are so minute, however, that it is evident Pappus's instrument resembled closely that of Gay-Lussac. It was, however, provided with two scales, one with its numbers increasing upwards to indicate the volume submerged in liquids of different density; the other with its numbers increasing downwards, to show the specific gravities corresponding to those submerged volumes.

The above-mentioned Pappus was an eminent Greek geometer of Alexandria, who flourished about 380 or 400 A.D. Consequently he was a contemporary of Synesius of Cyrene (378—430 A.D.), in one of whose letters occurs what is ordinarily regarded as the first recorded mention of the hydrometer. It is certainly most interesting to find that al-Khazini's description of Pappus's instrument corresponds very closely with the statements of Synesius—a coincidence not observed by previous writers.

Synesius, "the good bishop of Ptolemais," writing to his instructress,* the fair Pagan philosopher and mathematician, the ill-fated Hypatia, and being desirous of trying the wines† he is using, says—

"My health is so delicate that I need a *hydroscope*, and I beg you to have one made for me of copper. It is a tube, cylindrical in shape, and of the form and size of a pipe; on its length it bears a straight line crossed by small lines, by means of which we determine the weight of waters. One end is terminated by a cone, arranged in such a manner that the tube and the cone have the same base. This instrument is called *baryllion.* If you place it in water, point downwards, it stands erect, and the divisions that cross the vertical line can be easily counted, and by this means the density of water is determined."*

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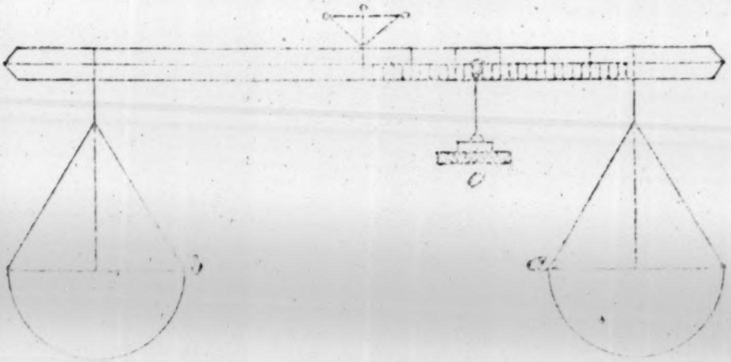
* Not in a letter of Hypatia to Synesius, as Hoefer has it in his Hist. Physique; Paris, 1872, 12mo.

† DRAPER, Hist. Int. Devel. Europe.

Hoefer, the French Historian of Chemistry, in relating this statement, remarks that none of the commentators of the Letters of Synesius were able to explain the nature of this instrument until the mathematician Fermat, in answer to Castelli's request, communicated his view, correctly apprehending the principles and uses of the instrument described. This was in 1628, and now we learn that the Arabian philosophers five centuries earlier were perfectly familiar with the identical instrument mentioned by Synesius.

FIG. 3.—BALANCE OF ARCHIMEDES.



a. Bowl for gold. *b.* Bowl for silver. *c.* Movable weight

Al-Khazini describes several forms of balances at great length, giving details of construction and employment. One of these balances he ascribes to Archimedes; and he professes to quote the particulars respecting it from Menelaus, without however giving the title of the latter's work.

Another balance described by our author is that of Muhammed Bin Zakariya of Rai; it differs from that of Archimedes by the introduction of the needle, called by the Arabs the "tongue," and by the substitution of a movable suspended bowl for the movable weight.

Finally, in the fifth lecture, he gives a minute description of the Balance of Wisdom according to Abu-Hatim al-Muzaffar Bin Ismail, of Isfazar. "He begins by remarking that the balance being an instrument of precision, like astronomical instruments such as the astrolabe and the zijassafaih, its whole workmanship should be carefully attended to. He next describes the beam, the front piece, the two 'cheeks' between which the 'tongue' moves, and the tongue itself." He gives the length of the beam as 4 bazaar cubits (2 metres), and remarks that "length of the beam influences the sensibility of the instrument;" it is constructed of iron or bronze. The balance is provided with five bowls or pans, made of very thin plates of bronze, three of which have the form of hemispheres (see Fig. 4), one of which is spherical, and the remaining one, destined to be plunged into water, is finished with a conical bottom. Two of these bowls bore the name of the "aërial," and were permanently attached to the beam; another pan was movable on the right arm of the beam; and the bowl intended to be immersed in water was fastened underneath the aërial bowl of the left arm: this bowl bore the name of the "aquatic," and the spherical bowl was named the "winged."

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* Synesius Opera, Epist. XV., Lutetia, 1612, 4to, p. 174.

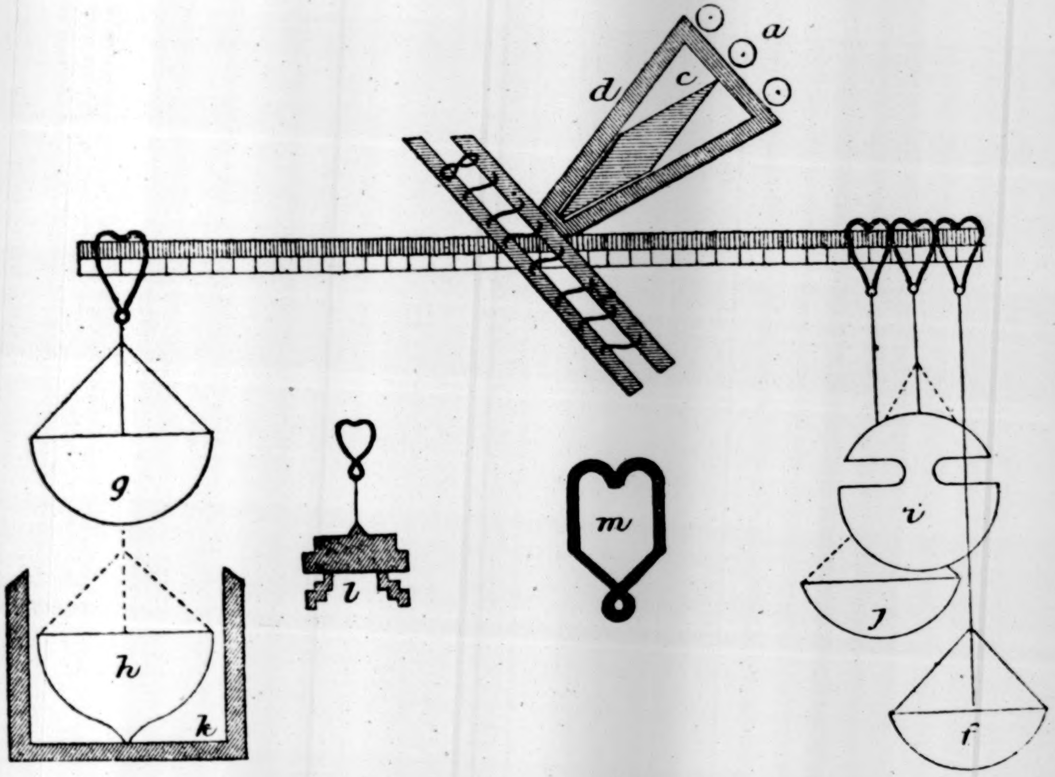
We cannot here enter upon a more detailed examination of this portion of al-Khazini's treatise; suffice it to say, he speaks of the mode of adjusting the balance and of its application to the examination of metals and of precious stones. Al-Khazini distinctly states that in taking the specific gravity of bodies he employed "a determined sort of water, similar in density to the water of the Jaihun of Khuwarazm," and further that "we made all our comparisons in one single corner of the earth, namely, in Jurjaniyah [a city] of Khuwarazm, ... and early in the autumnal season of the year." The "Jaihun" is the modern river Oxus, and "Khuwarazm" corresponds to the modern province of Khiva.

The editor, M. Khanikoff, calls special attention to the following passages, which he considers the most remarkable in the whole treatise:—

"When a heavy body, of whatever substance, is transferred from a rarer to a denser air, it becomes lighter in weight; from a denser to a rarer air it becomes heavier." —(Lect. I., Chap. v., § 1.)

"Air-weight does not apparently vary, although there is actual variation, owing to difference of atmospheres. As regards its water-weight, a body visibly changes according to the difference between waters of [different] regions, wells,

FIG. 4.—BALANCE OF WISDOM (*one-third the size of original drawing*).



a. Means of suspension.
c. Tongue.
d. Two cheeks.
f. Air-bowl.
g. Second air-bowl.
h. Water-bowl.
i. Winged bowl.
j. Movable bowl.
k. Basin.
l. Pomegranate counterpoise.
m. Rings of suspension.

and reservoirs, in respect to rarity and density, together with the incidental difference due to the variety of seasons and uses. So then the water of some determined region and known city is selected, and we observe upon the water-weight of the body, noting exactly what it is, relatively to the weight of 100 mithkals; and we refer [all] operations to that [result as a standard], and keep it in mind against the time when we are called upon to perform them, if the Supreme God so wills. In winter one must operate with tepid, not very cold water, on account of the inspissation and opposition to gravity of the latter, in consequence of which the water-weight of the body [weighed in it] comes out less than it is found to be in summer. This is the reason why the water-bowl settles down when the water has just the right degree of coldness, and is in slow motion, while in case it is hot and moving quickly, or of a lower temperature, yet warmer than it should be, the bowl does not settle down as when the water is tepid. *The temperature of water is plainly indicated, both in winter and summer;* let these particulars therefore be kept in mind."—(Lect. V., Chap. vi., § 5.)

An examination of these extracts compels a belief that the Arabian philosophers of the twelfth century knew the air to have weight, though they never applied the means they had discovered of measuring it. The sentence in italics leads to the conjecture that they also had some means of determining the temperature of water; possibly a form of aërometer was the instrument employed, and they were thus enabled to recognise the fact that the density of water increases in proportion to its coldness.

Al-Khazini's work contains several tables of the specific gravities of substances determined either by the Balance of Wisdom or by the hydrometer of Pappus. In these tables are enumerated fifty substances, nine of which are metals, ten precious stones, thirteen materials of which models were made, and eighteen liquids. The smallness of the list is not surprising, for most of the substances contained in modern lists of specific gravities were entirely unknown to the Arabians; the exactness of the results obtained is marvellous, when we take into consideration the coarseness of their means of graduating instruments and the backward state of the mechanical arts at that period.

The first table comprises the specific gravities of seven metals and two alloys; the results, interpreted into our system, together with the values assigned by modern authorities,* are found below. For sake of comparison, we also annex the figures obtained by Abu-r-Raihan, from whom it is believed al-Khazini quotes; the slight discrepancies are largely due to different methods of calculation adopted by Clément-Mullet and Khanikoff.

AL-KHAZINI'S FIRST TABLE OF SPECIFIC GRAVITIES.

Substances.	Al-Khazini.	Abu-r-Raihan.	Modern Authorities.
Gold [cast] .	19·05	19·05	19·30
Mercury . .	13·56	13·58	13·59
Lead . . .	11·32	11·33	11·34
Silver	10·30	10·35	10·52
Bronze . . .	8·82	8·82	8·05 to 8·95
Copper . . .	8·66	8·70	8·78
Brass . . .	8·57	8·57	8·58
Iron (forged) .	7·74	7·74	7·79
Tin	7·32	7·31	7·29

It is interesting to learn that the Arabian physicists fully appreciated the necessity of operating on pure materials, and the advantages of averaging the results of many determinations.

Thus al-Khazini says he purified gold by melting it five times, after which it melted with difficulty, solidified rapidly, and left hardly any trace upon the touchstone; and, after ten trials to obtain the weight of the volume of water displaced by different weights of the gold, he found, for a hundred mithkals of gold, weights varying from 5 mithkals 1 danik and 1 tassuj to 5 mithkals 2 daniks; as mean weight he adopts 5 mithkals 1 danik 2 tassujs, which by calculation yields the figures in the preceding table.

Likewise mercury was purified by passing it repeatedly through many folds of linen cloth. In writing of mercury, he remarks that it is not, properly speaking, a metal, but it is "the mother of the metals, as sulphur is their father." This view of the nature of mercury was prevalent among Arabian chemists, and is found in the writings of Geber (or Djafar), who lived four centuries earlier. Geber writes of mercury—"It is also (as some say) the matter of metals with sulphur,"† and he does not place it in the same class with metals which he defines as "extensible under the hammer," a property not possessed by mercury under ordinary conditions.

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* Cf. Prof. F. W. CLARKE's Tables of Specific Gravities, &c., in Constants of Nature, Part I., Smithsonian Miscellaneous Collections, 1873, 8vo.

† GEBER, Sum of Perfection, Book I., Part III., Chap. 6.

In this conception of al-Khazini we find, moreover, the germs of the doctrine of the transmutation of metals, the basis of that alchemical pseudo-science which subsequently acquired such a wonderful influence over the human race. For if metals have mercury for a mother and sulphur for a father, they are not simple substances, and if compounds, they are capable of artificial preparation and mutual transformation. This is, however, not the only passage containing allusions to a belief in transmutation, though no mention occurs of any practical attempts to effect it; the following extract clearly refers to the compound nature of metals.

"When the common people hear from natural philosophers that gold is the most equal of bodies, and the one which has attained to perfection of maturity, at the goal of competition in respect to equilibrium, they firmly believe that it is something which has gradually come to that perfection by passing through the forms of all [other metallic] bodies, so that its gold nature was originally lead, afterwards became tin, then brass, then silver, and finally reached the perfection of gold."

Writing of the precious metals, al-Khazini discourses in a philosophical spirit on their universal appreciation, in the following language:—

"'Men prize these metals,' says Abu-r-Raihan, 'only because under the action of fire they admit of being made into conveniences for them, such as vessels more durable than others, instruments of agriculture, weapons of war, and other things which no one can dispense with who is set to possess himself of the good things of life and is desirous of the adornments of wealth.' But if besides the rarity of the occurrence of gold, its durability, and the little appearance of moisture upon it, whether moisture of water or humidity of the earth, or of its being cracked or calcined by any fire, and consumed together; with its ready yielding to the stamp, which prevents counterfeiters from passing off something else for it, and lastly the beauty of its aspect—if there is not [besides all these characteristics] some inexplicable peculiarity pertaining to gold, why is the little infant delighted with it, and why does he stretch himself out from his bed in order to seize upon it? And why is the young child lured thereby to cease from weeping, although he knows no value that it has, nor by it supplies any want? And why do all people in the world make it the ground of being at peace one with another, not drawing their swords to fight, though at the sacrifice of the powers of body and soul, of family connections, children, ground possessions, and everything, with even a superfluity of renunciation for the sake of acquiring that; and yet are ever longing for the third stream[*] to stuff their bellies with the dust?"

This passage is a sample of the simplicity of much of the "Book of the Balance of Wisdom;" and, occurring in the midst of the purely scientific demonstrations and data, is peculiarly refreshing; the author's testimony to that inexplicable peculiarity of gold which renders it the special object of avarice, leads us to conjecture that had he lived in modern times he would have proved a warm champion of "hard money" doctrines.

The second table of specific gravities contains the determinations of various precious stones; it is not possible in every case to identify the stones, and hence some uncertainty obtains with regard to the values:—

Substances.	Specific Gravities.	
	Al-Khazini.	Modern Authorities.
Celestial hyacinth	3·96[*]	4·83
Red hyacinth	3·85[+]	3·99
[Ruby] of Badakhshan . .	3·58	—
Emerald	2·75	2·68 to 2·77
Lapis Lazuli	2·69	2·90
Fine pearl	2·60	2·68
Cornelian	2·56	2·62
Coral	2·56	2·69
Onyx and crystal	2·50	2·63[‡] to 2·88[§]
Pharaoh's glass	2·49	2·45[‖] to 3·44[**]

Al-Khazini gives detailed accounts of these precious stones, of which we quote a few brief extracts. He says:—"Emerald and chrysolite are interchangeable names, whether applied to one and the same thing, or to two things of which one has no real existence," a passage which shows that mineralogical terminology was afflicted with superfluous synonyms at an early day. Of the cornelian he says:—"Men have long tired of the cornelian, so that it has ceased to be used as a stone for seal-rings; even for the hands of common people, to say nothing of the great." Of the "fine pearl," he writes:—"The pearl is not a stone at all, but only the bone of an animal, and not homogeneous in its parts."

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[*] Alluding, say the editors, to the traditional saying of the Arabians: "If the son of Adam were to possess two flowing rivers of gold and silver, doubtless he would desire a third."

[*] Oriental sapphire.

[+] Oriental ruby.

[‡] Onyx 2·63 to 2·81.

[§] Rock crystal 2·68 to 2·88.

[‖] English mirror glass.

[**] English flint glass.

"Coral," he writes, "is a plant, though petrified like the Jew's stone and the sea-crab." He is aware that "glass is not the product of a mine, but, on the contrary, kindred to stones, or sand, or alkali," and he states that he has included it in the above list, "because it resembles crystal."

The third table comprises "the materials of models and patterns formed by goldsmiths, and woods of well-known trees." Interpreting the Arabic weights as before, we have the following table:—

Substances.	Specific Gravities.	
	Al-Khazini.	Modern Authorities.
Clay of Siminjan	1·99	clay 1·068 to 2·63
Pure salt.	2·19	2·068 to 2·17
Saline earth	1·11	—
Sandarach	0·71	1·05 to 1·09
Amber	0·85	1·06 to 1·08
Enamel	3·93	—
Pitch	1·04	1·07
Wax	0·95	0·96
Ivory	1·64	1·82
Ebony	1·13	1·18
Pearl shell	2·48	2·64
Bakkam wood	0·94	1·03
Willow wood	0·40	0·58

The last table embraces a number of liquids.

Substances.	Specific Gravities.	
	Al-Khazini.	Modern Authorities.
Sweet water	1·000	1·000
Hot water	0·958	0·959
Ice	0·965	0·916 to 0·926
Sea water	1·041	1·028 to 1·040
Water of Indian melon	1·016	—
Salt water [saturated solution]	1·144	1·205
Water of cucumber	1·017	—
Water of common melon	1·030	—
Wine vinegar	1·027	—
Wine	1·022	1·013
Oil of sesame	0·915	0·992 to 1·038
Olive oil	0·920	0·917 to 0·919
Cow's milk	1·110	1·029 to 1·040
Hen's egg	1·035	1·090
Honey	1·406	1·450
Blood of men in good health	1·033	1·053
Warm human urine	1·018	1·011
Cold human urine	1·025	

The temperatures at which the determinations of the "hot water" and "sweet water" were made are not known; the difference of density observed—viz., 0·04166—approximates that between water at 3·9° C. and 100° C., which according to modern physicists is equal to 0·04044.

The high specific gravity of cow's milk is noteworthy, and may have led al-Khazini and others into lactometrical controversy.

Besides these contributions to the knowledge of specific gravities, al-Khazini devotes some attention to certain subjects not closely connected with the main theme. In the third lecture he attempts to calculate the quantity of gold which would compose a sphere equal to the globe of the earth, and arrives at a number of mithkals which requires for expression 29 digits.

In the same lecture he takes up the problem of the chess-board, of which he supposes the squares to be filled with dirhams, each square containing twice the number in the preceding: he finds the total number of dirhams to be 18,446,744,073,709,551,615. He then applies himself to ascertaining the dimensions of the treasury in which the treasure should be deposited, and finally quotes the lines of an Arabian poet which fix the time in which one might spend this sum at 200,000,000,000,000,000 years.

In the last lecture he describes the methods of applying the balance to levelling and to the measuring of time. Of this portion M. Khanikoff gives the following concise exposition:—

"The balance level consists of a long lever, to the two ends of which were attached two fine silken cords, turning on an axis fixed to a point a little above its centre of gravity, and suspended between two sight-pieces of wood, graduated. At the moment when the lever became horizontal, the cords were drawn in a horizontal direction, without deranging its equilibrium, and the divisions of the scales of the sight-pieces corresponding to the points where the cords touched them were noted. For levelling plane surfaces, use was made of a pyramid with an equilateral triangular base, and hollow and open to the light, from the summit of which hung a thread ending with a heavy point. The base of the pyramid thus arranged was applied to the plane which was to be levelled, and carried over this plane in all directions. Wherever the plane ceased to be horizontal the joint deviated from the centre of the base.

"The balance-clock consisted of a long lever suspended similarly to the balance-level. To one of its arms was attached a reservoir of water, which, by means of a small hole perforated on the bottom of it, emptied itself in twenty-four hours. This reservoir, being filled with water, was poised by weights attached to the other arm of the lever, and, in proportion as the water flowed from it, the arm bearing it was lifted, the weights on the other arm slid down, and by their distance from the centre of suspension indicated the time which had elapsed."

Further analysis of the contents of this extraordinary work is incompatible with our reader's patience, and yet many points of interest demand at least a passing notice: these may, however, be embodied in a summary of the principal propositions contained in this treatise, and the recapitulation may serve to justify in some measure this contribution to the early history of chemical physics:—

1. The "Book of the Balance of Wisdom" shows the Arabian philosophers of the twelfth century to have entertained advanced views regarding attraction. They recognised gravity as a force, and attributed to it a direction towards the centre of the earth; they also knew that it diminishes with the distance, but they erroneously supposed this diminution to be in the direct ratio of the distance and not as its square.
2. They were acquainted with the connection between the weight of the atmosphere and its increasing density, since mention is made of the loss of weight of a body weighed in a denser atmosphere.
3. They understood the theory of centre of gravity, and applied it to the investigation and construction of balance and steelyards.
4. They made frequent use of the hydrometer, which they inherited from antiquity, and possibly they employed this instrument as a thermometer for distinguishing by variations of density the different temperatures of liquids.
5. They observed the action of capillary attraction.
6. They compiled full and accurate tables of the specific gravities of most of the solids and liquids with which they were acquainted.
7. Their system of philosophy was founded on experiment and observation.

In conclusion, we quote the following appropriate remarks from M. Khanikoff's introduction:—

"The history of the sciences presents to us an incontestable fact of deep significance—the re-discovery in modern times of truths laboriously established of old; and this fact is of itself enough to indicate the necessity of searching carefully in the scientific heritage of the past after all that it may be able to furnish us for the increase of our actual knowledge; for a double discovery, necessarily requiring a double effort of human intellect, is an evident waste of that creative force which causes the advance of humanity in the glorious path of civilisation."

VI. THE PHILOSOPHY OF THE RADIOMETER AND ITS COSMICAL REVELATIONS.

By W. Mattieu Williams, F.R.A.S., F.C.S.

So much speculation, and not a little extravagant speculation, has been devoted to the dynamics of the radiometer, that I feel some compunction in adding another stone to the heap, my only apology and justification for so doing being that I propose to regard the subject from a very unsophisticated point of view, and with somewhat heretical directness of vision,—*i.e.*, quite irrespective of atoms, molecules, or ether, or any other specific preconceptions concerning the essential kinetics of radiant forces beyond that of regarding such forces as affections or conditions of matter which are transmitted radially in constant quantity, and therefore obey the necessary law of radial diffusion, or inverse squares.

The primary difficulty which appears to have generally been suggested by the movements of the radiometer, is the case which it seems to present of mechanical action without any visible basis of corresponding reaction; a visible tangible object pushed forward, without any visible pushing agent or resisting fulcrum against which the moving body reacts.

This difficulty has been met by the invocation of obedient and vivacious molecules of residual atmospheric matter, which have been called upon to bound and rebound between the vanes and the inner surfaces of the glass envelope of the instrument.

How is it that the advocates of these activities have not sought to verify their speculations by modifying the shape and dimensions of the exhausted glass bulb or receiver? If the motion of the radiometer is due to such excursions and collisions, the length of excursion and the angles of collision must modify its motions; and such modification under given conditions would form a fine subject for the exercise of the ingenuity of molecular mathematicians. If their hypothetical data are sound, they should be able to predict the relative velocities or torsion force of a series of radiometers of similar construction in all other respects, but with variable shapes and diameters of enclosing vessels.

If we divest our minds of all visions of hypothetical atoms, molecules, ethers, &c., and simply look at the facts of radiation with the same humility of intellect as we usually regard gravitation, this primary difficulty of the radiometer at once vanishes. The force of gravitation is a radiant force acting somehow between, or upon, or by distant bodies; and these bodies, however far apart, act and react upon each other with mutual forces, precisely equal and exactly contrary. We conceive the sun pulling the earth in a certain direction, and receiving from the earth an equal pull in a precisely contrary direction, and we have hitherto demanded no ethereal or molecular link for the transmission of these mutually attractive forces. Why, then, should we not regard radiant repulsive energy in the same simple manner?

If we do this there is no difficulty in finding the ultimate reaction fulcrum of the radiometer vanes. It is simply the radiating body, the match, the candle, the lamp, the sun, or whatever else may be the source of the impelling radiations. According to this view, the radiant source must be repelled with precisely the same energy as the arms or pendulum of the radiometer; and it would move backward or in opposite direction if equally free to move. If, by any means, we cause the glass envelope of the radiometer to become the radiant source, it should be repelled, and may even rotate in opposite direction to the vanes or *vice versa*. This has been done with floating radiometers.

Viewed thus as simple matter of fact, irrespective of any preconceived kinetics of intervening media, the net result of Mr. Crookes's researches become nothing less than the discovery of a new law of nature of great magnitude and the broadest possible generality, viz., that the sun and all other radiant bodies, *i.e.*, all the materials of the universe, exert a mechanical repulsive force, in addition to the calorific, luminous, actinic, and electrical forces with which they have hitherto been credited. He has shown that this force is refrangible and dispersible, that it is outspread with the spectrum, but is most concentrated, or active, in the region of the ultra-red rays, and progressively feeblest in the violet;

or otherwise stated—it exists in closer companionship with heat than with light, and closer with light than with actinism.

According to the doctrine of exchanges, which has now passed from the domain of theory to that of demonstrated law, all bodies, whatever be their temperature, are perpetually radiating heat-force, the amount of which varies, *cæteris paribus*, with their temperature. If we now add to this generalisation that all bodies are similarly radiating mechanical force and suffering corresponding mechanical reaction, the difficulties vanish. What must follow in the case of a freely suspended body unequally heated on opposite sides?

It must be repelled in a direction perpendicular to the surface of its hottest side. If two rockets were affixed to opposite sides of a pendent body, and were to exert unequal ejective forces, the reaction of the stronger rocket would repel the body in the opposite direction to its preponderating ejection. This represents the case of the radiometer vane with one side blackened and the other side bright. When exposed to luminous rays, the black side becomes warmer than the bright side, by its active absorption and conversion of light into heat, and thus the blackened face recedes.

We may regard it thus as reacting by its own radiations, or otherwise as acted upon by the more powerful radiant whose rays are differentially received by the black and bright sides. These different modes of regarding the action are perfectly consistent with each other, and analogous to the the two different modes of regarding gravitation, when we describe the sun as attracting the earth, or, otherwise, the earth as gravitating to the sun. Strictly speaking, neither of these descriptions is correct, as the gravitation is mutual, and the total quantity exerted betwen the sun and the earth is equal to the sum of their energies, but it is sometimes convenient to regard the action from a solar standpoint, and at others from a terrestrial. So with the radiometer and the strictly mutual repulsions between it and the predominating radiant.

It appears to me that this unsophisticated conception of radiant mechanical repulsive force, and its necessary mechanical reaction on the radiant body, meets all the facts at present revealed by the experiments of Mr. Crookes and others.

The attraction which occurs when the disc of the radiometer is surrounded with a considerable quantity of atmospheric matter is probably due to inequality of atmospheric pressure. The absorbing face of the disc becomes heated above the temperature of the opposite face, the film of air in contact with the warmer face rises, leaving a relatively vacuous space in front. This produces a rush of air from back to front which carries the radiometer vane with it. When the exhaustion of the radiometer is carried so far that the residual air is only just sufficiently dense to neutralise the direct repulsion of radiation, the neutral point is reached. When exhaustion is carried beyond this, repulsion predominates.

Taking Mr. Crookes's estimate of the mechanical energy of solar radiation at 32 grains per square foot, 2 cwts. per acre, 57 tons per square mile, &c., and accepting these as they are offered, *i.e.*, merely as provisional and approximate estimates, we are led to a cosmical inference of the highest importance, one that must materially modify our interpretations of some of the grandest phenomena of the universe. Although the estimated sunlight pressure upon the earth, the three thousand millions of tons, is too small a fraction of the earth's total weight to effect an easily measurable increase of the length of our year, the case is quite otherwise with the asteroids and the zones of meteoric matter revolving around the sun.

The mechanical repulsion of radiation is a superficial action, and must, therefore, vary with the amount of surface exposed, while that of gravitation varies with the mass. Thus the ratio of radiant repulsion to the attraction of gravitation goes on increasing with the subdivision of masses, and becomes an important fraction in the case of the smaller bodies of the solar system. A zone of meteorites travelling around the sun would be broken up, sifted, and sorted, into different orbits according to their diameters, if this superficial repulsion operated against gravitation without any compensating agency. Gravitation would be opposed in various degrees, neutralised, and even reversed, in the case of cosmic dust. Comets presenting so large a surface in proportion to their mass, would either be driven away altogether or forced to move in orbits utterly disobedient to present calculations. This would occur if the interplanetary spaces were as nearly vacuous as the torsion instrument with which Mr. Crookes made his measurements.

Regarding the properties of our atmosphere only in the light of experimental data, irrespective of imaginary molecules, and their supposed gyrations or oscillations, we see at once that an inter-planetary or inter-stellar vacuum must act like a Sprengel pump upon our atmosphere, upon the atmosphere of other planets, and upon those of the sun and the stars, and would continue such action until an equilibrium between the repulsive energy of the gas and the gravitation of the solid orbs had been established. Atmospheric matter would thus be universally diffused, with special accumulations around solid orbs, varying in quantity with their respective gravitating energy. Such a universal atmosphere would accelerate orbital motion, and this acceleration would vary with the surface of bodies. Its action being thus exactly opposed to that of radiant repulsion, it must, at a certain density, exactly neutralise it. That it does this is evident from the obedience of all the elements of the solar system to the calculated action of gravitation, and thus Mr. Crookes's researches not only confirm the idea of universal atmospheric diffusion, but they afford a means by which we may ultimately measure the actual density of the universal atmosphere. If, as I have endeavoured to show, in my essay on "The Fuel of the Sun," the initial radiant energy of every star depends upon its mass, and its consequent condensation of atmospheric matter, the density of inter-planetary atmosphere sufficient to neutralise the radiant mechanical energy of our sun may be the same as is demanded to perform the same function for all the stars of the universe, and all their attendant worlds, comets, and meteors.

In order to prevent misunderstanding of the above, I must add that I have therein studiously assumed a negative position, in reference to all hypothetical conceptions of the nature of heat, light, &c., and their modes of transmission, simply because I feel satisfied that the subject has hitherto been obscured and complicated by overstrained efforts to fit the phenomena to the excessively definite hypotheses of modern molecular mathematicians. The atoms invented by Dalton for the purpose of explaining the demonstrated laws of chemical combination, performed this function admirably and had great educational value, so long as their purely imaginary origin was kept in view, but when such atoms are treated as facts, and physical dogmas are based upon the assumption of their actual existence, they become dangerous physical superstitions. Regarding matter as continuous, *i.e.*, supposing it be simply as it appears to be—and coextensive with the universe, in accordance with the experimental evidences of the unlimited expansibility of gaseous matter; we need only assume that our sensations, and the other phenomena of heat, light, &c., are produced by active conditions of such matter analogous to those which are proved to produce our sensations, and the other phenomena of sound. On this basis there is no difficulty in conceiving the rationale of the reaction which produces the repulsion of the radiometer. I may even go further, and affirm that with this basis to our philosophy, it is impossible to rationally conceive radiation without mechanical reaction. If heat be motion, and actual motion of actual matter, mechanical force must be exerted to produce it, and a body which is warmer on one side than the other, *i.e.*, which is exerting more outward motion-producing force on one side than on the other, must be subject to proportionally unequal reaction, and therefore, if free to move, must retreat in a direction contrary to that of its greater activity. Regarded thus the residual air of the radiometer does act, not by collisions of particles between the vane and inside of the glass vessel, but by the direct reaction of the radiant energy which would operate irrespective of vessels, *i.e.*, upon naked radiometer vanes if carried half way to the moon, or otherwise freed from excess of atmospheric embarrassment.

The recent experiments of Mr. Crookes, showing retardation of the radiometer with extreme exhaustion, seem to indicate that heat-rays, like the electric discharge, demand a certain amount of atmospheric matter as their carrier.

I cannot conclude these hasty and imperfect notes, written merely with suggestive intent, without quoting a passage from the preface to the "Correlation of Physical Forces," which, though written so long ago, appears to me worthy of the profoundest present consideration.

"It appears to me that heat and light may be considered as affections; or, according to the undulatory theory, vibrations of matter itself, and not of a distinct ethereal fluid permeating it: these vibrations would be propagated just as sound is propagated by vibrations of wood or as waves by water. To my mind all the consequences of the undulatory theory flow as easily from this as from the hypothesis of a specific ether; to suppose which, namely, to suppose a fluid *sui generis* and of extreme tenuity penetrating solid bodies, we must assume, first, the existence of the fluid itself; secondly, that bodies are without exception porous; thirdly, that these pores communicate; fourthly, that matter is limited in expansibility. None of these difficulties apply to the modification of this theory which I venture to propose: and no other difficulty applies to it which does not equally apply to the received hypothesis."

NOTICES OF BOOKS.

The Origin of the Stars, and the Causes of their Motions and their Light. By JACOB ENNIS. London: Trübner and Co. New York: D. Appleton and Co.

THIS book offers us a solution of some most difficult questions. In his prefatory address the author directs the attention of astronomers to the following discoveries contained in his book:—He pronounces that gravitation is not merely the force which now holds the stars in their orbits, but which first set them in motion. He adds "a catalogue of some of the recent discoveries which have sprung from our knowledge of the projectile or centrifugal force." This catalogue we venture to quote:—

"1. The demonstration that the nebular theory of the origin of the universe is true: not the nebular theory of Laplace, but a theory far more comprehensive, and now first unfolded in this volume.

"2. The discovery that both the centripetal and centrifugal forces in astronomy, although antagonistic, are in all cases due to the same origin, the force of gravity.

"3. The discovery that gravity was the force which originally gave all their peculiar arrangements to all stellar systems, whether they be sidereal, solar, or planetary. It determined not only the velocities of the celestial bodies, but the direction of their movements, the size and positions of their orbits, and even their separate existences.

"4. The reason why the four outer planets have all the satellites but one, and why of the four inner planets the Earth alone has a satellite.

"5. Why there is no planet interior to Mercury.

"6. Why the distance of the innermost planet, Mercury, is 35,000,000 miles from the sun, whilst the distance of the innermost satellites is only about a quarter of a million miles from their primary planets.

"7. Why the interplanetary spaces are so large, from 32,000,000 miles to 1,000,000,000 miles, while the intersatellitic spaces are only from 134,000 to 1,300,000 miles.

"8. Why Saturn has so many more satellites and rings than Jupiter.

"9. Why the interplanetary spaces become less as we approach the sun, and why the last interplanetary space is an exception.

"10. Why the same facts and the same exceptions occur in the systems of Saturn and Uranus.

"11. Why the orbits of the planets and satellites have become elliptical.

"12. The reason why the orbital planes of the planets are not in the equatorial planes of the sun; why the orbital planes of the satellites are not in the equatorial planes of the planets; why the axes of the planets and satellites are not perpendicular to their orbital planes; and why the rotation of Uranus, and possibly that of Neptune is retrograde.

"13. Why the inner rings of Saturn remain unbroken.

"14. The explanation of the ring of the asteroids, probably many hundred small plants in a belt by themselves.

"15. The explanation of the origin of the milky way.

"16. Why all planetary and solar systems move rapidly through space, and why all sidereal systems are stationary."

The author also seeks to demonstrate the cause of stellar light and heat to be chemical action. From his remarks on this subject we take the following extracts:—"The ignition of meteors proves that our air extends considerably higher than 205 miles. Even before reaching that point the amount of latent heat in the upper regions of the air is something wonderful. When this very rare air is compressed in front of a swiftly-shooting meteorite, the repulsion is overcome and the heat becomes sensible. The air is then heated to ignition, and often vitrifies the surface of the meteorite. In my paper on 'Meteors,' published in the 'Proceedings of the American Association for the Advancement of Science,' 1871, I have given examples of meteors which have been known to travel horizontally a thousand miles and more through the upper strata of the atmosphere, and continue vividly bright through the whole distance. I gave also examples of other meteorites which came down nearly vertically, and, although in broad daylight they glowed when high up as brightly as the sun, nevertheless their light was invariably extinguished before reaching the earth. Had the meteors themselves been incandescent, their brightness would have continued another twinkling of the eye until they struck the earth. Meteors are bright in the upper regions of the air, because of the abundance of latent heat up there. Their brightness goes out in the lower and denser strata of the air, because there is much less latent heat, although the *friction* is so much greater." He then contends that it is impossible for us to say how much chemical force can be stored up in the unknown chemical elements in the sun, or in their compounds. He declares that "the very greatest of all the lessons taught by the spectroscope has hitherto been overlooked by scientific men; this lesson is the wide difference between the earth and the sun as regards their simple elements." Spectroscopists also let slip another truth, which is that "there are thousands of fixed lines in the solar spectrum which are perfect strangers to us."

In short, the author contends that the sun contains many elements not present in our earth, and that, such being the case, all computations on the length of time during which a mass of matter like the sun could continue to burn are necessarily illusive.

His remarks on the assumed identity of matter throughout the universe are not without interest to chemists. It is plain that he does not regard our present chemical elements as essentially and primordially distinct bodies, but as mere modifications or compounds of materials as yet unknown.

We have thus laid before the reader some specimens of this extraordinary work. Space will not permit us to give the evidence adduced by the author in support of his positions, especially as it is of a cumulative nature. We by no means see our way to a general acceptance of his conclusions, and we entertain strong doubts concerning some of his fundamental facts. To the following passage we take decided exception :—" It (the objection to the chemical theory of stellar light and heat) assumes, thirdly, that no new chemical elements can be formed in the sun,—that the materials of the sun cannot be decomposed or metamorphosed so as to be burnt over again. Here we can decompose water, and burn the oxygen and hydrogen a hundred times ; but we lose as much force or heat in the decomposing as we gain in the burning. But in the sun the case may be very different." We can as readily conceive that in the sun two and three are equal to six, as that a compound there can be decomposed without a consumption of force equal to that which was liberated during its composition. We have carefully read the section in which the author undertakes to prove that new combustibles may be prepared in the sun from matter which has already been burned ; but we cannot say that he has made out his case.

But for all this we are far from condemning the work before us. The author seeks not to overturn, but to extend and supplement the work of his predecessors in astronomy and physics, and even where he—in our opinion—has failed his labours cannot fail to prove useful.

The Moon, and the Condition and Configurations of its Surface. By EDWARD NEISON, F.R.A.S. London: Longmans and Co. 1876.

A BOOK on the moon, dealing specially with selenographical relations, is needed. In fact, lunar literature is incomplete for want of such a work. Nasmyth's interesting treatise on the moon, besides being too costly for the observatory, deals rather with theoretical than practical selenography ; and Proctor's treatise is general, not selenographical. There was room, therefore, for a treatise devoted specially to this department of lunar research. Unfortunately Mr. Neison has not been content to produce such a treatise. He has gone over ground already well-trodden, and while his book has thus been so enlarged as to be as costly as Mr. Nasmyth's, it has been deprived of the special qualities which would have made it valuable to selenographers. It is, in fact, a general treatise on the moon.

The first chapter deals with the motions, figure, and dimensions of the moon; the second with her physical condition; the third with the various lunar formations; the fourth with what the author calls "lunar history," meaning thereby the history of lunar research; the fifth with the variations of the moon's surface; the sixth introduces the description of illustrative maps, to which the seventh, and many following chapters relate in detail; and the last chapter presents a number of formulæ, selenographical and otherwise. The chapter on the motions, figure, and dimensions of the moon is not satisfactory. For the most part, Mr. Neison quotes the descriptions given by French and German mathematicians. In some cases, by the way, he does not translate these descriptions correctly, as, for instance, at page 6, where the word "remplacer," used in the sense of "taking the place of," is represented by the English word "replace," which has quite a different, and, in this case, a quite incorrect sense.

In describing the motion of the lunar perigee, Mr. Neison writes, "The direction of the semi-axis major is not constant, but undergoes a slow revolution, the period of which is 8·8505 years, and in the same direction as the motion of the moon in its orbit, but with variable velocity, the lunar perigee being occasionally before and then behind its mean place." This description is incorrect. The motion of the perigee is not always in the same direction as the motion of the moon in her orbit and variable only in velocity. It varies in direction also, being sometimes progressive and sometimes retrogressive; advancing, on the whole, because the progressions are greater than the retrogressions. The reverse is the case with the motion of the nodes, which alternately retreat and advance, but on the whole retreat; whereas Mr. Neison describes this motion also as simply varying in velocity. The second chapter, on the moon's physical condition, is more original, but, apart from errors respecting physical laws, there is one remarkable error relating to a subject which Mr. Neison claims to have studied with special attention—the optical laws, namely, in which the refractive action of an atmosphere depends. "It may be remarked," he says, "that the rays, after traversing any atmosphere to the moon, would not be convergent as in a lens, but, owing to the refraction diminishing rapidly with the distance from the surface would be truly divergent; for the rays refracted by each spherical shell of atmosphere, as it were, of indefinably small thickness, would reach a different focus." According to this novel way of dealing with the matter, the rays of a direct pencil refracted through a lens would be regarded as convergent or divergent, according as the aberration was towards or from the second surface of the lens, which we submit to be an entirely new view of convergency and divergency. Having, by reasoning satisfactory to himself, demonstrated the existence of a lunar atmosphere having probably about 1-300th part of the surface density of our atmosphere, Mr. Neison states that such an atmosphere is "not incapable of exerting as powerful influences on the surface as the earth's," and this because "its mass in proportion to that of the planet is only a little less than a fourth of that of the earth's, and with regard to even a single square mile in area of the surface, must be estimated by millions of tons." But in what way the largeness of the total mass of the atmosphere, or the pressure on a square mile of lunar surface, is to compensate for the fact that the surface density and pressure are certainly very minute, he in no way attempts to show. If the atmospheric pressure could be reduced anywhere on the earth till the mercurial barometer stood at one-tenth of an inch, the amount of air somewhere else, or the fact that even this reduced pressure was equivalent to some 900 millions of tons on the square mile, would not in the slightest degree tend to make that attenuated air equal in influence to air 300 times as dense.

The chapter relating to the history of lunar research is fairly accurate. Towards the close, however, Mr. Neison refers to the work done under the auspices of the British Association without mentioning the fact, that this work gave so little promise of leading to anything of value that the British Association withdrew its support. It is the general opinion of astronomers that the outlay of the Association was entirely thrown away.

The chapter on variations of the moon's surface contains little that has not been said elsewhere; but there is one passage relating to the supposed changes of the floor of Plato which is certainly incorrect. After describing these changes, Mr. Neison says that they are not "due to the effects of contrast, remaining unaffected when these are eliminated." The exact reverse is the case.

The next section, which indeed forms the bulk of the work, is occupied by descriptions of twenty-two maps. These remind us of Mädler's large map reduced in surface more than one half, cut up into twenty-two parts, and modified by omissions and additions, the law of which we fail to recognise. Some details are added which only the possessors of powerful telescopes could hope to examine with advantage, others are omitted which can be easily seen with a three inch object-glass. But the great mistake has been the division of the chart into many parts and the introduction of 3-into a cumbrous volume. The selenographer does not want a book of maps, still less a large volume in which such maps are accompanied by a quantity (in this case nearly 600 pp.) of letterpress, but a chart which he can hang in his observatory.

With respect to the chapter headed Selenographical Formulæ, the remarks we have made on the first chapter seem entirely applicable, with this additional one, that a large proportion of the formulæ are not selenographical at all, and could have no value whatever, except to astronomers specially engaged in studying the lunar motions, who already possess these formulæ in books belonging to their subject.

Mr. Neison's book, necessarily large from the multitude of matters which he has undertaken to deal with in it, is made larger by repetitions. For instance, on page 133 a passage is repeated which occurs on page 132.

There is one point not peculiar to this book, though illustrated by it, which we should hardly deem important enough to notice were it not that it has attracted the attention of Continental astronomers, and been made the subject of specially unfavourable comment in America (in an article in the "Nation" for November, 1873, attributed, correctly we believe, to Prof. Newcomb, of the Washington Observatory). We refer to the nomenclature. When Beer and Mädler raised the number of names of lunar craters and other features to 427, one-third of which they added themselves, it was felt that the thing had been a good deal overdone, though the important contributions made by Beer and Mädler to lunar research prevented astronomers from expressing that opinion very strongly. But since Beer and Mädler's chart was published a number of new names have quite unnecessarily been added by Mr. Birt, and others, in such a way as to make the whole system of lunar nomenclature ridiculous. It ought to be understood that only such selenographers as Schröter, Lohrmann, Beer and Mädler, and Schmidt,—men who have constructed new and valuable lunar charts, not mere drawings of small lunar regions,—should venture to add new names to lunar maps, even if new names were wanted. As it is, one lunar student adds a set of names which he communicates to another (whose name is included); this one approves of them and adds other names, including that of his friendly correspondent. Mr. Birt sends his new list of names to Mr. Webb, who adds them to his already too lengthy list. Then Mr. Webb's name appears in a new list by Mr. Birt, and so on *usque ad nauseam*. We may be certain that neither American nor Continental astronomers, nor any English astronomers of repute, will adopt these new names,—at least until they have some selenographical contributions of far more importance from Mr. Birt than mere promises and a portfolio of lunar scraps.

The Argonaut. Edited by G. GLADSTONE, F.R.G.S., F.C.S. London: Hodder and Stoughton.

THE "Argonaut" is a periodical differing from others of its class by the relatively large share of space it devotes to Science. Still we seem, in turning over its pages, to breathe a strange atmosphere. Much of the volume, too, lies fairly outside our sphere of criticism. Thus we find a series of papers on the "Use of the Supernatural in Art." We doubt not the ability here displayed, but in how far the author's views are sound we cannot venture to pronounce.

In a paper on the "Birth of Alchemy," by Prof. Gladstone, F.R.S., we find this strange pursuit traced much further eastwards than Egypt or Arabia, namely, to China, where it flourished from the sixth century before Christ. A work on the preparation of the Elixir, entitled the "Uniting Bond,"* was written by Wei-peh-yang during the Eastern Han Dynasty, some time from A.D. 25 to 221. An epitome of this work has been published by the Rev. Dr. Edkins in the "Miscellany or Companion to the Shanghai Almanack for 1857." We extract the following passage, which shows a strong resemblance to the alchemical writings of Western Asia and of Europe:—"The cauldron is round, like the full moon, and the stove beneath is shaped like the half moon. The lead ore is symbolised by the white tiger, which, like metal among the elements, belongs to the West. Mercury resembles the sun, and forms itself into sparkling globes. It is symbolised by the blue dragon, belonging to the East, and it is assigned to the element wood. Gold is imperishable. Fire does not injure its lustre. Like the sun and the moon, it is unaffected by time. Therefore the elixir is called the golden elixir. Life can be lengthened by eating the herb called *hu-ma*; how much more by taking the elixir which is the essence of gold, the most imperishable of all things. . . . Lead-ore and mercury are the basis of the process by which the elixir is prepared. They are the hinge on which the principles of light and darkness revolve."

The Elixir was not, however, as we may judge from its materials, found very successful in practice. Indeed two Emperors of the Tang Dynasty are said to have died from its effects—a result which very naturally ruined its *prestige*. The whole story has a double interest: not merely does it throw a new light on the origin of one of the strangest phases of the chemistry of the past, but it proves that China was not always the swamp of stagnant learning to which she has been reduced by her system of competitive examinations. Would that we might take the lesson to heart! Surely everyone can see that the man best able to "get up" a subject on short notice is not necessarily the man most capable of original thought!

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* Have we here an anticipation of the notions of certain modern theorists?

Prof. Barrett's article, "The Jubilee Singers," is objectionable as favouring by insinuation the vulgar belief that the distinction between the Aryan and the Ethiopian "races" lies in the colour of the skin,—a belief actually dangerous, since it leads some who should know better to apply the insulting and resented epithet "niggers" to our Hindoo fellow-subjects, or to include them along with the negro under the cant phrase "people of colour."

"Reminiscences of Holland" is a series of papers highly laudatory of the Dutch. Holland—by affording, as she once did, an asylum for persecuted thinkers, and a place where their works might be printed without the interference of the censorship—has merited well of the world. But there is a terrible set-off: she originated national debts — and chicory! The "Orthodox" party in Holland, a body somewhat kindred to the English "Evangelicals," sum up all the results of modern thought as "the lies," thereby not merely pronouncing it erroneous, but branding its advocates with insincerity. This reminds us of Prof. J. W. Dawson, who, in one of his invectives against the new Natural History, speaks of those who "affect" to be unable to recognise marks of design in the organic world. Does the learned Professor find insult sometimes a more convenient weapon than argument?

The reports on the "Progress of Science," including physics, geology, biology, chemistry, geography, and astronomy, are ably and fairly compiled.

We have especial pleasure in recognising the candid manner in which the doctrine of Evolution is handled. It is time the outside public made the discovery that there is nothing necessarily atheistic in the idea of a transmutation of organic forms.

Improvement of the Condition of Workmen. By T. Egleston, Ph.D. Philadelphia: Sherman and Co.

"Improvement of the condition of workmen," but how? High wages have been tried, with the simple and unsatisfactory result that the money is carried straightway to the tavern. Reduced hours of labour have tended greatly to the promotion of dog-races, pigeon-matches, and even to the revival of cock-fighting. Arrangements for mere physical welfare, such as are carried out at the Hôtel Louise, of the Hassard Collieries, near Liege, are, as Prof. Egleston remarks, only "insignificant items, so far as the general well-being of the men is concerned. Something requires to be done to elevate their moral as well as physical condition." But what is this something? The author describes the arrangements he has found in operation in different countries of Europe with different degrees of success. On returning to

America he hoped to put some of these schemes into practice, and "commenced, with more enthusiasm than judgment, to apply a system drawn from my experience in England and the Continent to Celts, with not very fortunate results. They were suspicious of the houses that I built; were certain that I was personally deriving benefit from the fines that I proposed for the foundation of their fund; were jealous of their own members, raised to higher position than themselves, as they thought, by being brought more in contact with the officers of the Company than they were; and caused so much trouble that in the interest of the Company I was obliged to abandon any attempts to elevate the condition of the men, and I must confess that I have been obliged as conscientiously to abstain from acting as I was at first disposed conscientiously to act."

This is a very discouraging result, and the suspicions of the men were no doubt ridiculous. Yet we must remember that the hated "truck system," which has enriched so many contractors and the like, has often been disguised under attempts to promote the welfare of the employed. Many judicious politicians even think that all connection between the employer and his workmen beyond the actual hours of labour should, on this account, be discouraged as much as possible by stringent legal enactments. Just as there are men whose only endeavour is to do their work as badly as they dare, and get through the week with the minimum amount of exertion, physical or mental, so there are masters who regard their men as natural enemies, to be outwitted and cheated if possible. Yet for a number of men to act successfully in concert without mutual respect and goodwill is in the long run impossible. In industry, as in war, the most perfect *matériel* is of little avail if the *personnel* be untrustworthy. Confidence between employer and employed, based on a conviction that either party is desirous to do his best for the other, is the first step towards an improvement in the condition of workmen; but there is no receipt for creating such confidence. The man who can inspire it, like the true poet, *nascitur, non fit.*

Building Construction, showing the Employment of Brickwork and Masonry in the Practical Construction of Buildings. By R. SCOTT BURN. London and Glasgow: W. Collins, Son, and Co.

THE present work, otherwise unimpeachable of its kind, is strangely included in an "Advanced Science Series,"—an instance of a confusion very annoying to the methodologist, but to which the British public seem incurably given. The author treats on plan-drawing; on the employment of brick in construction; on the varieties of stone-work; foundations, walls, arches, sewers, drains, tanks, and wells; on concrete building, &c. The work is profusely illustrated, and will be of great value to those engaged in the study of the constructive arts.

The State of the Medical Profession in Great Britain and Ireland. By WILLIAM DALE. Being the successful Carmichael Prize Essay in 1873. Dublin: J. Atkinson and Co.

FROM the Preface to this Essay we have great pleasure in extracting the following passage:—

"Certain restless women have sought recognition as medical probationers or students, of the different corporations; especially have they, with wonderful persistence, forced their so-called claims upon the attention of the University of Edinburgh and the General Medical Council. The former of these bodies has emphatically said 'No' to these bold aspirants after medical work and medical fame; but the latter, by the half-hearted mode in which it has dealt with the question, has opened the door to future trouble, and given grave offence to the profession. A short time ago, at Heidelberg, we saw two young female students going round the wards of the hospital with the young men. The strange sight arrested instant attention, but the exhibition sadly repaid it. They stood behind the male students, and seemed to have no place in the class; indeed how could they? Look at the case under observation. It is a case of Ascites in a man! What could gallantry, or good nature, or anything else do for them? Where could they be placed but behind?"

If we look further into the pamphlet, and note how overcrowded the profession evidently is, we shall yet further question the wisdom of those who seek to assist women in obtaining medical qualifications.

The following account is true, and "pity 'tis 'tis true":—"We see men with diplomas keeping open retail shops, and selling almost everything that the druggist sells, hair-oil, scented soap, perfumery, tobacco and cigars, patent medicines, &c. We see others having open surgeries, one remove above shops, with their glaring red lamps, as trade-marks to attract customers. We see others jostling each other in the strife for existence, advertising and underselling like any Cheap Jack of the day; seeking to visit patients and supply medicines for 1s. 6d., 1s., or even 6d. In a shop-window not more than a stone's throw from Westminster Abbey the writer saw medical attendance and medicine advertised at even a lower rate than this." The Medical Club system and the Poor-Law service give equally convincing proofs of the unsatisfactory state of the medical profession,—a state as dangerous to the public as it is degrading to the practitioner. The new sphere of sanitary work created by the Public Health Act has scarcely, as our author hopes would be the case, brought "fresh emolument and honour to the profession." In some districts, where there is abundant work, the "Medical Officer of Health" receives a salary of £10 yearly, on the understanding that he is not to interfere with any source of disease and death.

Mr. Dale's remedy for the repletion and consequent degradation of the profession is that the present multiplicity of examining and licensing boards should cease, and that there should be only one portal through which all should enter, and that under more stringent regulations. The author's proposals deserve careful consideration, and would be as a whole beneficial, though we cannot endorse them in detail.

Blue- and Sun-Lights; their Influence upon Life, Disease, &c. By General A. J. PLEASANTON. Philadelphia: Claxton and Co. London: Trübner and Co.

WE have here a book printed in blue ink, bound in blue, and treating on the chemico-physiological action of the blue ray of light. The author's fundamental experiment is as follows:—A vinery was built in 1861. Its dimensions, situation as regards the points of the compass, &c., are minutely described, and present nothing remarkable. But every eighth row of glass in the roof was of violet glass, the rows on opposite sides alternating. The mould had no peculiarity, and the vines, of twenty different sorts, were planted in the usual manner in the month of April. In the early part of September, the same year, some of the vines were found to be 45 feet in length, and an inch in diameter at a foot from the ground, whilst similar vines, planted about the same time, had only reached the length of 5 feet. In September, 1862, the vinery yielded 1200 lbs. of grapes. The remark is added that "in grape-growing countries a period of from five to six years will elapse before a single bunch of grapes can be produced from a young vine." The next season (1863) the yield of grapes was 2 tons, the vines being perfectly healthy and free from disease. "So on, year by year, the vines have continued to bear large crops of fine fruit without intermission for the last nine years. They are now healthy and strong, and as yet show no signs of decrepitude or exhaustion."

Similar experiments have been made on poultry, pigs, and on a calf, and in all cases the result is described as being analogous —accelerated maturity, increased size, weight, and vigour.

Now we do not for a moment wish to accuse General Pleasanton of trifling with the public, and we can scarcely see how the enthusiasm to which all discoverers are liable can have led him astray in matters which are determined by weight and measure. His experiments, too, are very easily capable of verification. Still we are bound to say that his results accord very ill with those of other and earlier experimentators. Thus Prof. H. Vogel—certainly no mean authority on the chemical action of light—states, in his "Chemistry of Light and Photography" (p. 78), that "recent observations have established that the yellow and red rays, and not the blue and violet, produce the greatest chemical effect on the leaves of plants." Dr. R. Hunt, in that well-known work the "Poetry of Science," fully admits that seeds under blue glass will germinate long before others exposed to ordinary daylight, whilst under the yellow ray the process of germination is entirely checked; but he resumes—"If the experiment is continued it will be found that under the blue glass the plants grow rapidly, but weakly, and that, instead of producing leaves and wood, they consist chiefly of stalk, upon which will be seen here and there some abortive attempts to form leaves. When the process of germination has terminated, if the young plant is brought under the yellow glass it grows most healthfully, and forms an abundance of wood, the leaves having an unusually dark green colour, from the formation of a large quantity of chlorophyl. Plants do not, however, produce flowers with readiness under this medium; but if, at the proper period, they are brought under the red glass, the flowering and fruiting processes are most effectively completed."

In confirmation Dr. Hunt quotes a letter from Mr. C. Lawson, of Edinburgh, an eminent seed-merchant. This gentleman, as early as 1853, had proved the value of blue light in accelerating germination, and employed it practically in testing the value of the seeds coming into his hands in the course of business. He found that seeds could be thus caused to germinate in two to five days, instead of, as heretofore, in eight to fourteen; but he adds that he has "always found the violet ray prejudicial to the growth of the plant *after* germination."

Here, then, is a complete discrepancy, and either General Pleasanton on the one hand, or Messrs. Vogel, Hunt, and C. Lawson on the other, must be decidedly mistaken. In all such cases the only expedient is further experiment, which in this case certainly involves no delicate manipulation, and might be satisfactorily performed by any intelligent horticulturist. One point of difference between General Pleasanton's arrangements and those of the European experimentators upon the influence of the various rays of light upon organic life is, that the latter—ourself included—submitted plants and animals to the sole and exclusive action of blue, yellow, or red light respectively, whilst in General Pleasanton's experiments the blue light has been used mixed in certain proportions with ordinary daylight.

It appears that the author applied for a patent for his discovery, and that Prof. Brainerd was deputed by the Commissioners of

Patents to investigate the reality of the alleged results. After a minute inspection he is reported to have declared that all General Pleasanton's statements concerning the action of blue light were confirmed.

But if General Pleasanton is in the right, the wonderful and salutary effects of blue light upon organic life is by no means the most extraordinary of its properties. Heat is also, in some unaccountable way, developed in the passage of sunlight through blue glass. We read—"During the winter of 1871-72, which in this city was a very cold and rigorous one, two ladies of my family, residing on the northern side of Spruce Street, east of Broad Street, in this city,—who, at my suggestion, had caused blue glass to be placed in one of the windows of their dwelling, associated with plain glass,—informed me that they had observed that when the sun shone through these associated glasses in their window, the temperature of the room, though in mid-winter, was so much increased that on many occasions they had been obliged during sunlight to dispense entirely with the fire which ordinarily they kept in their room, or, if the fire was suffered to remain, they found it necessary to lower the upper sashes of their windows, which were without the blue glass, in order to moderate the oppressive heat." Further, a "distinguished German scientist," not named, is quoted as declaring that "one-half of the fuel heretofore required can be saved by thus utilising sunlight." Yet in an earlier portion of the work we find the author expressing the opinion that a vinery roofed with blue glass would be cooler than one constructed of uncoloured glass. "Should too much (blue glass) be used it would reduce the temperature too much." It need scarcely be said that experimentalists have not found the blue and violet rays of the spectrum to be the hottest portions.

We should feel much greater confidence in General Pleasanton's observations if he had been content to place them simply before the world as novel and—if verified—important facts; but he goes much farther, and deduces from them an entire new philosophy. Into these his doctrines it will be early enough to examine when the action of blue light shall have been satisfactorily ascertained.

Hydraulic Experiments at Roorkee, 1874-5. By Capt. ALLAN CUNNINGHAM, R.E. Roorkee: Thomason College Press.

AN experimental examination of surface and sub-surface velocities in rivers, flowing canals, &c.

A Course of Practical Chemistry arranged for the Use of Medical Students. By W. Odling, M.B., F.R.S. Fifth Edition. London: Longmans and Co.

This is a new and improved edition of Dr. Odling's well-known treatise on chemical manipulation, qualitative analysis, toxicology, and animal chemistry. The former portion has been carefully revised by Dr. John Watts, and the latter by Dr. T. Stevenson, of Guy's Hospital.

At the present time it is more than ever imperative upon medical students to make themselves perfectly acquainted with the action of poisons upon the animal system, and to acquire at least a rudimentary knowledge of toxicology. Taking its cue from certain recent cases of a somewhat sensational character, a portion of the lay press is labouring assiduously for a triple object—to create a panic on the subject of secret poisoning; to proclaim, by implication at least, eminent medical and chemical authorities as deficient in their knowledge of the symptoms and the behaviour of poisons; and to demand increased restrictions upon the sale of substances of a deleterious character. It is darkly hinted that were inquiries as careful as that in the Balham case more common, many deaths now accepted as "natural" would be removed into another and more alarming category. It is insinuated that physicians cannot or do not distinguish the symptoms of natural disease from the effects of poisons, and that chemical analysis is in complicated cases not to be depended on.

We cannot accept these views. For the frequency of secret poisoning there is no proof, but merely the random surmise of "smart writers" wishful to create a sensation in the so-called "silly season." As regards the alleged incompetency of men of science, it must be remembered that it was not the medical and chemical practitioners who broke down in the "Balham mystery." The medical experts did not fail to recognise in the symptoms exhibited by the patient the effects of an irritant poison. The chemist detected that poison as present in a fatal quantity. If, therefore, a failure of justice has here occurred,—which it is not our province to examine.—it is surely idle in the extreme to make of the Bravo tragedy a peg on which to hang scandalous charges against men of science.

As to increased restrictions upon the sale of poisons, we must —in the name both of chemical research and chemical manufactures—protest against such a proposal. If we are to banish from the workshop every substance which cannot be swallowed with impunity, our trade will indeed be imperilled. Not only are deadly drugs in constant and necessary use in the factory, but poisons of the most formidable character can be gathered in wood, field, and garden. Further, even if all poisons could be kept out of the hands of malicious persons, nothing would be gained. Of what use is it, as Milton asks in his "Samson

Agonistes," to guard one gate of a city and leave another open to the enemy? What is the use of severe "Pharmacy Acts" as long as explosives and fire-arms may be purchased and kept by all sorts and conditions of men? It is all very well for a daily paper to indulge in the statement that "an ounce of cyanide of potassium is capable of doing more harm than a ton of nitroglycerine." Even were this true, the writer overlooks the main point—that explosives can be used to produce such sweeping havoc as the Bremerhaven and the Clerkenwell outrages, whilst even the deadliest poison, in the hands of the most remorseless villain, can prove fatal only to those few persons to whose food he has access; a fact which terribly narrows the circle within which the guilty person must be sought. We must therefore denounce, as utterly uncalled for, the proposal to increase the stringency and the scope of the Acts regulating the sale of poisons. It is a great misfortune that when political and literary organs find themselves compelled to deal with some scientific topic, they do not see the propriety of putting the matter into the hands of some specially qualified writer.

Annual Record of Science and Industry for 1875. Edited by SPENCER F. BAIRD. London: Trübner and Co.

WE have here a condensed summary of the most important inventions, discoveries, and observations made during the year 1875.

Among the paragraphs thus collected we are first struck, though most unfavourably, with a "new mode of embalming," said to have been discovered by one Madame Jalourear. The corpse is to be placed in an impermeable coffin, together with certain substances which produce a rapid, though not putrid, decomposition, whilst the products cannot escape from the enclosure. The substance in question is phosphate of lime, which is said to have the property of causing rapid decomposition without offensive odour. To begin with, we doubt this very much. We have always found soluble phosphates—and no less insoluble ones, if in a gelatinous state—powerful promoters of putrefaction in its most offensive form, whence the necessity, in the treatment of sewage, of withdrawing all phosphates from solution. It is further a grievous fault that all the nitrogen, phosphates, and other principles found in the bodies of the dead should be withdrawn from natural circulation, and that an additional quantity of phosphate of lime should be wasted in the attempt to modify the character of decomposition. The scheme holds, in our opinion, a doubtful position between folly and crime.

In a paper on the influence of the roots of living vegetables upon putrefaction, Jeannel pronounces "the fact that cemeteries, bogs, and marshes are made salubrious by vegetation indisputable, being purely the result of experience." Would M. Jeannel kindly show how this theory adapts itself to such districts as the Terai, the Gold Coast, the Tierras Calientes of Mexico, &c., where we have at once the most luxuriant vegetation and the most intense insalubrity?

The intellectual capacity and educability of children of different "races"—to say species would be, we suppose, a capital offence—has been studied by M. Houzeau. He concludes that there is in each child a different rate of intellectual proficiency, but that such differences amount to less than might be anticipated, and that an unequal rate of improvement does not belong specially to any one race. Nay, as we have seen it remarked elsewhere, the inferior races even seem to have an advantage. On this fact, however, the very just comment has been made by other observers that this advantage is extremely transitory.

The existence of gigantic cuttles is now no longer a matter of question. One, whose larger arms measured each 26 feet in length and 16 inches in circumference, was cast ashore on the Grand Bank, Fortune Bay, in December, 1874. The entire length of its body was 14 feet.

As another wonder of the deep may be mentioned a marine worm 300 yards in length, discovered by Dr. Carl Möbius, off Mauritius. Is not this quite as extraordinary as the much-disputed "sea-serpent"?

Closet naturalists have sometimes maintained that man has at all times and in all places, except where debarred by climate, selected the same animals for domestication, the inference being drawn that no others were capable of being truly and permanently tamed. This theory is disproved by evidence obtained from Egyptian monuments. Several species of antelope were formerly bred and kept in a state of domestication, such as the gazelle, the kobe, addax, and beisa. In the pictures on Egyptian tombs "flocks of these animals are represented receiving the attentions of the farmer and the herdsman." From about 1800 B.C. these representations become fewer and fewer in number, and ultimately disappear. It would be interesting to ascertain whether their removal from the ranks of tame animals was due to the conclusion that they were not remunerative, or to a general decline in the art of husbandry.

It has been justly remarked that man's power of destroying noxious animals, from the tiger or the cobra down to the locust and the mosquito, has increased in a far less rapid ratio than his means of inflicting death and destruction upon his own species. In the former department there is indeed great room for inventive ingenuity. Two species of *Pyrethrum* (*P. carneum* and *roseum*) have been for some time in use for destroying, or at least banishing, noxious insects, under the name of "Persian powder." The *P. cinerariafolium*, a Dalmatian species, is now found to be more active, and is coming into extensive use. We have been informed, however, that—as far at least as gnats and mosquitoes are concerned—the marsh rosemary (*Ledum palustre*) is more effectual than any species of *Pyrethrum*. As the *Ledum* flourishes in all boggy parts of Central and Northern Europe, and, if we are not mistaken, of Labrador, Canada, and the Hudson's Bay countries, the supply may be considered unlimited. It has been also alleged that the common brake fern (*Pteris aquilina*) is a nuisance to all insects. This we cannot admit, at least when the plant is in its recent state. We see butterflies and moths resting upon it; we have found its leaves eaten by caterpillars, and we never observe that those parts of woods where it is most luxuriant are less infested with flies than localities where it is absent.

An interesting observation, made by Professors Lartet, Gervais, Cope, and Marsh, is that the brains of extinct Mammalia have evidently been much smaller than are those of the most closely analogous species at the present day. "In the lines of the rhinoceros, tapir, and horse, a regular increase in size from such beginnings can be traced."

It would be alike impracticable and unfair were we to extend further our quotations, but we think the few specimens we have given will be amply sufficient to show the interesting character of the work.

Science Papers, chiefly Pharmacological and Botanical. By DANIEL HANBURY, F.R.S. Edited, with Memoir, by JOSEPH INCE, F.L.S., F.C.S., &c. London: Macmillan and Co.

PHARMACY, high as its claims may fairly be pronounced, is by no means popular. It can point to none of those triumphs which so forcibly appeal to the public imagination, such as the solution of phosphates, the discovery of the coal-tar colours, or the invention of malleable glass; nor is it connected with any of those dazzling speculations which are so enthusiastically welcomed by the young, however their foundations may fail to satisfy the old. It has to rely for appreciation upon sound, conscientious, valuable work, which attracts little notice from those whom it benefits. We need not wonder, therefore, that the author of these papers and the subject of this memoir is little known to the British public. Daniel Hanbury was not one of the class—unhappily now so common, and, more unhappily still, so much fostered by our institutions—who will, at a few days' notice, "get up" any subject, and write about it a showy superficial dissertation. He was no writer of "Manuals," repeating for the twentieth time fully-recognised facts, or perhaps current errors. He was a specialist who, confining his attention to what some, perhaps, may deem a limited sphere, was within that sphere satisfied with nothing short of perfection. Whatever he did bears the royal stamp of thoroughness. Never was he content with hearsay information if by any amount of labour it was possible to go to the fountain-head. As his friend and fellow-labourer Professor Flückiger observes,—" In countless instances second-hand knowledge could not stand its ground before his critical acumen, and had to give way before his superior observations. Only the most trustworthy reports, samples, and specimens collected with the greatest care by a skilful hand satisfied him. The best book-knowledge offered for sale in the market did not content him, but he referred back to the sources of information, testing them minutely." These attributes appear most strikingly manifested in the " Pharmacographia," the joint work of Flückiger and himself, a compilation which—" from the amount of its original matter, its laborious verification of facts, the accuracy of its references, and the extent of general erudition it reveals "—is perfectly unapproachable. It is to be regretted that a career so fruitful in the choicest results should have been brought to so early a close.

The volume before us includes a memoir of the deceased *savant*, his scientific papers, his addresses and miscellaneous papers, and an obituary notice from the pen of Dr. Flückiger. The editor has performed his task well, though we regret to find that he embraces and advocates the dangerous error that a life of business can be at the same time a life of scientific research. To this view we may refer elsewhere.

The Chemistry of Light and Photography in their Application to Art, Science, and Industry. By Dr. HERMANN VOGEL. London: H. S. King and Co.

THIS edition of Dr. Vogel's work is undoubtedly an improvement upon the foregoing. Most of the errors, chemical and otherwise, which disfigured the first edition have disappeared, and the English editor may be fairly congratulated upon having done his part satisfactorily.

But with the work itself we are not perfectly satisfied. It is certainly a valuable treatise upon photography, but the chemistry of light is here, in our opinion, very insufficiently treated. The title naturally leads us to expect that the behaviour of all known classes of bodies, natural and artificial, under the influence of light, and the effects of the solar rays in promoting or hindering combinations and decompositions, would have been at any rate briefly noticed. But into the chemical and bio-chemical action of light the author scarcely enters. Nay, in some cases, the few remarks which he makes on such subjects are scarcely fairly representative of the present state of science. Thus we read:—"The green colour of leaves and the variegated colours of flowers exist only under the operation of light. In the dark, plants only develop sickly blossoms, like the well-known white sprouts of potatoes kept in cellars."

The views here expressed are much too sweeping, and require to be modified. Thus Dr. Askenasy finds that the influence of light upon the colours of flowers varies greatly in different plants. The flowers of different varieties of *Tulipa Gesneriana*, whether yellow, scarlet and yellow, or scarlet and white, when grown in complete darkness, displayed no constant and appreciable difference in colour or intensity from flowers of the same respective kinds reared in the full light. The flowers of the common spring crocus, both blue and yellow, appeared in their natural colours, though not well developed. Upon other flowers, such as a dark blue hyacinth, the absence of light was found to produce more marked effects. Flowers grown in the light, though at the same temperature, were fully a fortnight earlier than such as were grown in darkness, and were more intensely coloured. Still the latter were not absolutely colourless. Thus it appears that the influence of light upon the development of vegetable and animal colouring matters, not to speak of other principles, is a subject which requires much more attention than it has yet received, and can by no means be cursorily dismissed. One of Dr. Askenasy's experiments is exceedingly interesting. Portions of the imperfectly coloured flower-spikes of several blue hyacinths, growing in the dark, were cut off and placed in glasses of water exposed to the full light on the south side of a greenhouse. In a few days these flowers had attained as full a shade as those which had been reared in full daylight from the first; which proves that the development of colour does not depend on a previous formation of chlorophyll.

These deficiencies are the more to be regretted because Dr. Vogel, in his Preface, if we do not misunderstand him, complains that "men of science have in great measure neglected this subject after the first enthusiasm."

On the other hand, some amount of space is taken up with matter which might well have been omitted. Thus we find an explanation of what is meant by the term "elements," which, now manuals of chemistry have been multiplied to such an extent, can scarcely be regarded as necessary.

The following passage gives room for reflection:—"Equally peculiar are the changes experienced in sunlight by two other elements not so well known, chlorine and bromine, which have only been carefully observed latterly." Chlorine and bromine, we should consider, may rank among the best known elements, and their combination with hydrogen gas under the action of sunlight—which, from the context, appears to be the peculiar change here referred to—has been carefully observed some time ago.

On the other hand, what may be called the strictly photographic portion of the work is very valuable.

The chapter on the "correctness of photographs," in which the author treats of the individuality of the photographer, of the influence of lenses, of the length of exposure, of colours and models, of the characteristic feature in the picture, of deviation from truth in photography, and of the difference between photography and art, may be read with great advantage by photographers, artists, and amateurs. Prof. Vogel does not countenance the popular notion that a photograph is necessarily and invariably more correct than a picture. We think that it is Nathaniel Hawthorn who maintains that the character of a person is more truly shown in his photographic likeness than in his face. That this may not have been incidentally the case we are not prepared to deny; but it is certainly not the rule. On the contrary, photography is apt to flatter one class of persons, and in revenge to be less than just to another. Among the latter are often found those whose principal attraction depends more on the play of expression than upon strict regularity of feature.

The author figures and recommends a photometer for determining the exact time needful for exposure. It will doubtless be evident that for this purpose the radiometer will probably prove highly valuable. The chapter on "photography in natural colours" gives an account of certain attempts made to solve this interesting problem, and recalls to our memory a strange hoax successfully perpetrated many years ago. It was announced that the art of taking coloured photographs had been discovered in America, and a descriptive catalogue of the specimens on their way for exhibition in London "went the round of the papers." But the specimens never arrived, and the whole affair proved to be merely a most unjustifiable falsehood.

The concluding chapter, on photography as a subject to be taught in industrial schools, is worth the serious attention of educational boards. Of course the term "industrial schools" is used here in its natural acceptation, and not in the strangely perverted meaning which has been forced upon it in England within the last few years.

The Principles of Dynamics; an Elementary Text-Book for Science Students. By R. WORMELL, D.Sc. London, Oxford, and Cambridge: Rivingtons.

THE author remarks, in his Preface, that "the object of the present work is to present and unfold the subject according to the method best suited for the purpose of the science student. It aims at supplying, without the aid of advanced mathematics, such explanations of the Laws of Dynamics as will prepare the way for their application to physical phenomena, particularly to those of Heat and Electricity."

In carrying out his undertaking, the author treats in succession of time and space, motion, force and motion, force and mass, momentum and energy, the laws of motion, the parallelogram of forces, moments of forces, reactions and surfaces, centres of gravity, energy, a system of particles, problems on energy, machines, friction, and moments of inertia. The descriptions and explanations are necessarily brief, but exceedingly lucid.

The work is plentifully illustrated with diagrams, and a series of questions is appended to every section. It may be safely recommended to students.

Check List of the Ferns of North America, North of Mexico. Published for John Robinson, Salem, Mass. The Naturalists' Agency.

This pamphlet is simply a Catalogue of all the ferns known to be indigenous in the United States and the Dominion. The total number of species is remarkably small in relation to the vast extent of territory. British fern collectors and cultivators will be interested to find that many of their old friends—such as *Osmunda regalis, Phegopteris dryopteris, Polypodium vulgare, Scolopendrium vulgare,* &c.—are common to both sides of the Atlantic.

Notes on Building Construction, arranged to meet the Requirements of the Syllabus of the Science and Art Department of the Committee of Council on Education. Part II.—Commencement of Second Stage, or Advanced Course. London, Oxford, and Cambridge: Rivingtons.

This second part treats of brickwork and masonry, timber roofs, roof-coverings, of beams and girders, of centres, joinery, stairs, rivetting fire-proof floors, iron roofs, plasterer's work, and painting. We should think that the artizan who has fairly mastered the contents of this work—we do not say who is able to "pass" in it—will very soon find the advantage, and will prove decidedly superior to those of his companions who are content to pass through life without caring at all for the fundamental principles of their work. The time has gone by when that mere mindless routine which some men call "practice" can suffice in any art. In the chapter on "graining" we regret to find that this national vice passes without a protest. It is not art, but simply falsehood, for which no rational plea can be urged.

Whilst pronouncing this book highly valuable to all connected with any of the building trades, we cannot help asking whether all other branches of industry are about to be dealt with as thoroughly and carefully, and if not, why not?

It is somewhat peculiar that the title-page does not bear the name of any author or editor.

Field Geology. By H. Penning, F.G.S. (H.M. Geological Survey of England and Wales). *With a Section on Palæontology*. By A. J. Jukes-Browne, F.G.S. (H.M. Geological Survey). London: Baillière, Tindall, and Cox.

Even in this age, so rich in manuals, hand-books, and text-books, it will happen that a science, under some very important aspect, may escape attention. Geology certainly has now been popular for a long time; its cultivators are numerous and increasing, and its literature is most comprehensive. Looking over the formidable array of published books, we might at first sight imagine that the subject had been already treated from every conceivable point of view. We have works theoretical and works practical, works elementary and works advanced, and yet till the appearance of the little volume before us one phase of geology appears to have been overlooked. Let us suppose a student who has made himself familiar with the science by reading, and wishes, as every student should, to become himself an observer. How is he to proceed? He has read of greensand, of oolitic limestone, of Kimmeridge clays, and the like, but the ordinary hand-books give him no clue as to how he is to know these and other rocks when he meets them. Here Mr. Penning steps in, and teaches him how and what to observe.

The author describes actual geological work under four heads —mapping, sections, the determination of rocks (lithology), and the observation of fossils (palæontology), and for each of these simple and practical instructions are given, the student being merely pre-supposed to have a fair general knowledge of the science, of the sequence of the various systems, formations, and groups, and of the general succession of fossil plants and animals. As far as possible the directions given are illustrated by examples of their practical applications. The section on the determination of rocks is exceedingly valuable. The author, from his extensive practical experience, is able to propose those methods which can be carried out in the field with the minimum of appliances. The student who follows his directions, and who has duly prepared himself by a careful study of cabinet specimens of the ordinary rocks and minerals, will soon feel at home in this department of geology. The author reverses the process often directed; he tabulates the results obtainable, and from them deduces the nature of the rock in question. The examination to which he subjects rocks in the field consists of noting the texture, fracture, lustre, behaviour with knife, effervescence with dilute acid, colour, and streak, the inferences drawn from these indications being afterwards verified at home by a determination of the hardness, specific gravity, solubility, and behaviour before the blowpipe. The examination of thin slices of rock under the microscope—of course impracticable in the field—is strongly and very justly recommended.

In the section on palæontology—the first three chapters of which are from the pen of Mr. Jukes-Browne—we meet with a remark which we would commend to the attention of those who are given to complain of the absence of "missing links:"—"Since the majority of rocks composing the crust of the earth are of aqueous and principally marine origin, we should naturally expect the fossils they contain to be the remains of aquatic and principally marine beings. Thus, in the Vertebrata, the remains of mammals and birds are among the rarest of geological relics, even in beds of littoral or terrestrial origin, save those of very recent date." This plain simple truth fully exposes the absurdity of drawing any conclusions from the absence of this or the other form of extinct life in the strata which we have been able to examine. The instructions given for collecting fossils bear the marks of experience and sound sense. The following "wrinkle" may be of value to the student:—"Chalk-fossils, and those which have been obtained from any similar porous limestone along the *sea-shore*, should be soaked in fresh water for several weeks, the water being changed at least once a week: this is the only way we know of to prevent the efflorescence of the salt in such cases, and the consequent splitting up of fossils which have cost time and pains to extract."

We find some very just remarks on the naming and arranging of fossils. The author laments the illusory ideas regarding the fixity of species, and the too great desire of finding something new to science "which too often takes the form of *species-making* instead of *discovery*." For this tendency he considers that "the only cure is to read Darwin's 'Origin of Species,' and persons who will not do that may be given up as hopeless."

We can decidedly recommend this work to the student as supplying a want of no mean importance.

PROGRESS IN SCIENCE.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE British Association has just concluded a highly successful meeting at Glasgow. The local arrangements were all that could be desired, while many of the papers read were of great scientific value. The number of members and associates present at the meeting was 2731.

In his Inaugural Address, the President—Dr. Andrews, F.R.S.—first gave a masterly review of the progress of science, but while he alluded at considerable length to the results obtained by scientific men in England, America, France, Germany, Italy, and Russia, he made too slight a reference to his own brilliant physico-chemical researches, which have added so much to our knowledge of the laws and composition of gases, to the nature and properties of ozone, and to the heat changes in chemical reactions, &c. In speaking of the North Polar Expedition, he referred to the opinion of those who hold that a full survey of the Arctic regions can never be of such value as to justify rhe risk and cost which must be incurred, and said that it was not by such cold calculations that great discoveries were made or great enterprises achieved. There was an inward and irrepressible impulse—in individuals called a spirit of adventure, in nations a spirit of enterprise—which impelled mankind forward to explore every part of the world we inhabit, however inhospitable or difficult of access; and if the country claiming the foremost place among maritime nations shrunk from an undertaking because it was perilous, other countries would not be slow to seize the post of honour. If it were possible for man to reach the poles of the earth, whether north or south, the feat must sooner or later be accomplished; and the country of the successful adventurers would be thereby raised in the sale of nations. He then alluded to the transit of Venus; to the confirmation by M. Cornu of Foucault's calculation of the distance of the earth from the sun; to Mr. Christie's confirmation of Dr. Huggins's discovery that some of the fixed stars are moving towards and some receding from our system; to Mr. Stone's confirmation of Prof. C. A. Young's observation that bright lines, corresponding to the ordinary lines of Fraunhofer reversed, may be seen in the lower strata of the solar atmosphere for a few moments during a total eclipse; to the observations of Roscoe and Schuster on the absorption bands of potassium and sodium; to Mr. Lockyer's investigation on the absorbtive powers of metallic and metalloidal vapours at different temperatures; to M. Lecoq de Boisbaudran's discovery of the new metal—gallium; to the discoveries and researches of Sir Edward Sabine, Nordenskiöld, Maskelyne, Lawrence Smith, Sorby, R. Apjohn, Daubrée, Wöhler, and Tschermak in connection with meteoric science.

In noticing the important services which the Kew Observatory has rendered to meteorology and to solar physics, Dr. Andrews expressed the hope that England would not lag behind in providing physical observatories on a scale worthy of the nation and commensurate with the importance of the object.

After a reference to the organisation in this country of a system of reporting by telegraph the state of the weather at selected stations to a central office, so that notice of the probable approach of storms may be given to the seaports, and also to Dr. Robinson's observations at the Observatory of Armagh, which led to the discovery that the mean velocity of the wind is greatest in the S.S.W. octant and least in the opposite one, and that the amount of wind attains a maximum in January, after which it steadily decreases with one slight exception, till July, augmenting again till the end of the year, Dr. Andrews passed to the subject of electricity; he announced the failure of a recent attempt to deprive Oerstedt of his great discovery. To Franklin, Volta, Coulomb, Oerstedt, Ampère, Faraday, Seebeck, and Ohm he ascribed the fundamental discoveries of modern electricity. In its applications the names of Davy, Wheatstone, Morse, and Thomson are prominent. In connection with the theory of electrical and magnetic action we find the names of Poisson, Green, Gauss, Weber, Helmholtz, Thomson, and Clerk Maxwell. Among recent inventions is mentioned Prof. Tait's discovery of consecutive neutral points in certain thermo-electric junctions; De la Rue and Müller's chloride of silver battery giving freely sparks through cold air, which, when a column of pure water is interposed in the circuit, accurately resembles those of the common electrical machine. The length of the spark increasing nearly as the square of the number of cells, it has been calculated that with 100,000 elements of this battery the discharge should take place through a distance of no less than eight feet in air.

Reference was made to Newton's grand investigation of the properties of light; to Lord Rosse's discovery that the surface of the moon facing the earth passes, during every lunation, through a greater range of temperature than the difference between the freezing and boiling points of water; to Prof. Stokes's discovery of the cause of epibolic dispersion, in which he showed that many bodies had the power of absorbing rays of high refrangibility, and of emitting them as luminous rays of lower refrangibility; to Mr. Crookes's experiments on repulsion caused by radiation; and to the discovery by Mr. Willoughby Smith of the power of light in diminishing the electrical resistance of selenium. This property has been ascertained to belong chiefly to the luminous rays on the red side of the spectrum. Lord Rosse observed that the action appeared to vary inversely as the simple distance of the illuminating power, and the accuracy of this observation has since been confirmed by Prof. W. G. Adams.

In welcoming General Menabrea as a distinguished representative both of the kingdom of Italy and Italian science, Dr. Andrews spoke of his great work on the determination of the pressures and tensions in an elastic system, the principle being stated in the following words:—"When any elastic system places itself in equilibrium under the action of external forces, the work developed by the internal forces is a minimum." He then referred to the mechanical integrator of Prof. J. Thomson, in which motion is transmitted, according to a new kinematic principle, from a disc or cone to a cylinder through the intervention of a loose ball; to Sir W. Thomson's machine for the mechanical integration of differential equation of the second order, and also to his tidal machine, by means of which the height of the tide at a given port can be accurately predicted for all times of the day and night.

The attraction-meter of Siemens was mentioned as an instrument of great delicacy for measuring horizontal attractions, which it is proposed to use for recording the attractive influences of the sun and moon, upon which the tides depend, and by means of Mr. Siemens's bathometer, in which the constant force of a spring is opposed to the variable pressure of a column of mercury, the depth of the sea may be approximately ascertained without the use of a sounding-line.

The President then remarked that it was often difficult to draw a distinct line of separation between the physical and chemical sciences; and it was doubtful whether the division was not really an artificial one. The chemist could make no large advance without having to deal with physical principles; and to Boyle, Dalton, Gay-Lussac, and Graham were due the discovery of the mechanical laws which govern the properties of gases and vapours. Some of these laws had of late been made the subject of searching inquiry, which had fully confirmed their accuracy, when the body under examination approached to what had not inaptly been designated the ideal gaseous state. But when gases were examined under varied conditions of pressure and temperature, it was found that these laws were only particular cases of more general laws, and that the laws of the gaseous state, as it exists in nature, although they might be enunciated in a precise and definite form, were very different from the simple expressions which apply to the ideal condition. The new laws became in their turn inapplicable when from the gaseous state proper we passed to those intermediate conditions which, it had been shown, linked with unbroken continuity the gaseous and liquid states. As we approached the liquid state, or even when we reached it, the problem became more complicated; but its solution even in these cases might confidently be expected to yield to the powerful means of investigation we now possess.

Among the more important researches made of late in physical chemistry, those of F. Weber on the specific heat of carbon and the allied elements, of Berthelot on thermo-chemistry, of Bunsen on spectrum analysis, of Wüllner on the band- and line-spectra of the gases, and of Guthrie on the cryohydrates were mentioned.

Cosmical chemistry abounded in facts of the highest interest. Hydrogen, which, if the absolute zero of the physicist did not bar the way, we might hope yet to see in the metallic form, appeared to be everywhere present in the universe. It existed in enormous quantity in the solar atmosphere, and it had been discovered in the atmospheres of the fixed stars. It was present, and was the only known element of whose presence we are certain, in those vast sheets of ignited gas of which the nebulæ proper are composed. Nitrogen was also widely diffused among the stellar bodies, and carbon had been discovered in more than one of the comets. On the other hand, a prominent line in the spectrum of the Aurora Borealis had not been identified with that of any known element; and the question might be asked—Does a new element, in a highly rarefied state, exist in the upper regions of our atmosphere? Or are we with Ångström to attribute this line to a fluorescent or phosphorescent light produced by the electrical discharge to which the aurora is due? This question awaited further observations before it could be definitely settled, as did also that of the source of the remarkable green line which is everywhere conspicuous in the solar corona.

Here Dr. Andrews paid a tribute to the memory of Ångström, whose great work on the solar spectrum will always remain as one of the finest monuments of the science of our period. The influence, he said, which the labours of Ångström and of Kirchhoff had exerted on the most interesting portion of later physics could scarcely be exaggerated; and it might be truly said that there were few men whose loss would be longer felt or more deeply deplored than that of the illustrious astronomer of Upsala.

Passing to the application of science to the useful purposes of life, Dr. Andrews referred to the application by Neilson of the hot-blast to the smelting of iron. The Bessemer steel process and the regenerative furnace of Siemens were later applications of high scientific principles to the same industry. But there was ample work yet to be done. The fuel consumed in the manufacture of iron, as, indeed, in every furnace where coal was used, was greatly in excess of what theory indicated; and the clouds of smoke which darkened the atmosphere of our manufacturing towns, and even of whole districts of country, were a clear indication of the waste, but only of a small portion of the waste, arising from imperfect combustion. The depressing effect of this atmosphere upon the working population could scarcely be overrated. At some future day the efforts of science to isolate, by a cheap and available process, the oxygen of the air for industrial purposes might be rewarded with success. The effect of such a discovery would be to reduce the consumption of fuel to a fractional part of its present amount; and although the carbonic acid would remain, the smoke and carbonic oxide would disappear. But an abundant supply of pure oxygen was not now within our reach; and in the meantime he would suggest that in many localities the waste products of the furnace might be carried off to a distance from the busy human hive by a few horizontal flues of large dimensions, terminating in lofty chimneys on a hillside or distant plain. A system of this kind had long been employed at the mercurial mines of Idria, and in other smelting-works where noxious vapours were disengaged. With a little care in the arrangements, the smoke would be wholly deposited, as flue-dust or soot, in the horizontal galleries, and would be available for the use of the agriculturist.

In speaking of organic chemistry, Dr. Andrews said that the discovery of quinine had probably saved more human life, with the exception of that of vaccination, than any discovery of any age; and he who succeeded in devising an artificial method of preparing it would be a true benefactor of the race. Not the least valuable, as it had been one of the most successful, of the works of our Government in India, had been the planting of the cinchona tree on the slopes of the Himalaya. As artificial methods were discovered, one by one, of preparing the proximate principles of the useful dyes, a temporary derangement of industry occurred, but in the end the waste materials of our manufactures set free large portions of the soil for the production of human food.

M. Dumas's method of destroying the *Phylloxera*, which lately threatened with ruin some of the finest vine districts in the South of France was then referred to. After a long and patient investigation, M. Dumas has discovered that the sulpho-carbonate of potassium, in dilute solution, fulfils every condition required from an insecticide, destroying the insect without injuring the plant.

The application of artificial cold to practical purposes was rapidly extending. The ice-machine was already employed in paraffin-works and in large breweries; and the curing or salting of meat was now largely conducted in vast chambers, maintained throughout the summer at a constant temperature by a thick covering of ice.

In completing his review, Dr. Andrews named the important work of Cayley on the "Mathematical Theory of Isomers," and to elaborate memoirs which had recently appeared in Germany on the reflection of heat- and light-rays, and on the specific heat and conducting-power of gases for heat, by Knoblauch, E. Wiedemann, Winkelmann, and Buff.

The latter part of the Address was devoted to the subjects of University Education and the Endowment of Research. A University, or *Studium Generale*, ought to embrace in its arrangements the whole circle of studies which involve the material interests of society, as well as those which cultivate intellectual refinement; and if, in accordance with the spirit of their statutes, or at least of ancient usage, the Universities would demand from the candidates for some of the higher degrees proof of original powers of investigation, they would, Dr. Andrews holds, give an important stimulus to the cultivation of science. The example of many continental universities, and among others of the venerable University of Leyden, was mentioned, and two proof essays recently written for the degree of Doctor of Science in Leyden —one by Van der Waals, the other by Lorenz, were referred to as works of unusual merit.

With regard to the endowment of research, Dr. Andrews refrained from discussing the subject as a national question, but considered that the universities ought never to be asked to give their aid to a measure which would separate the higher intellects of the country from the flower of its youth. It was only through the influence of original minds that any great or enduring impression could be produced on the hopeful student. Without original power, and the habit of exercising it, we might have able instructors, but we could not have great teachers. In every age of the world the great schools of learning have, as in Athens of old, gathered around great and original minds, and never more conspicuously than in the modern schools of chemistry, which reflected the genius of Liebig, Wöhler, Bunsen, and Hofmann. These schools had been nurseries of original research as well as models of scientific teaching; and students attracted to them from all countries became enthusiastically devoted to science, while they learned its methods from example even more than from precept. But while the universities ought not to apply their resources in support of a measure which would render their teaching ineffective, and would at the same time dry up the springs of intellectual growth, they ought to admit freely to university positions men of high repute from other universities, and even without academic qualifications. An honorary degree did not necessarily imply a university education; but if it had any meaning at all, it implied that he who had obtained it was at least on a level with the ordinary graduate, and should be eligible to university positions of the highest trust.

Dr. Andrews advocated that the English universities should recognise the ancient universities of Scotland as freely as they had always recognised the Elizabethan University of Dublin. If this union were established among the old universities, and if at the same time a new university—as he had earnestly proposed ten years ago—were founded on sound principles amidst the great populations of Lancashire and Yorkshire, the university system of the country would gradually receive a large and useful extension, and, without losing any of its present valuable characteristics, would become more intimately related than hitherto with those great industries upon which mainly depend the strength and wealth of the nation.

If Great Britain is to retain the commanding position she has so long occupied in skilled manufacture, the highest training which can be brought to bear on practical science will, Dr. Andrews said, be imperatively required; and it would be a fatal policy if that training had to be sought for in foreign lands, because it could not be obtained at home. The country which depended unduly on the stranger for the education of its skilled men, or neglected in its highest places this primary duty, might expect to find the demand for such skill gradually to pass away, and along with it the industry for which it was wanted. That education in its highest sense, based on a broad scientific foundation, and leading to the application of science to practical purposes—in itself one of the noblest pursuits of the human mind—could be most effectively given in a university, or in an institution like the Polytechnic School of Zürich, which differed from the scientific side of a university only in name, and to a large extent supplemented the teaching of an actual university, he was firmly convinced; and for this reason, among others, he had always deemed the establishment in this country of examining boards with the power of granting degrees, but with none of the higher and more important functions of a university, to have been a measure of questionable utility. It was to Oxford and Cambridge, widely extended as they could readily be, that the country should chiefly look for the development of practical science; they had abundant resources for the task; and if they wished to secure and strengthen their lofty position, they could do it in no way so effectually as by showing that in a green old age they preserved the vigour and elasticity of youth.

Dr. Andrews instanced the University of Berlin. It was founded in the year 1810, at a period when the pressure of foreign domination weighed almost insupportably on Prussia; and Dr. Hofmann had remarked that it would ever remain significant of the direction of the German mind that the great men of that time should have hoped to develop, by high intellectual training, the forces necessary for the regeneration of their country," and in his recent report on the artificial dyes, M. Wurtz said—" Let us not suppose that the distance is so great between theory and its industrial applications. This report would have been written in vain if it had not brought clearly into view the immense influence of pure science upon the progress of industry. If unfortunately the sacred flame of science should burn dimly or be extinguished, the practical arts would soon fall into rapid delay. The outlay which is incurred by any country for the promotion of science and of high instruction will yield a certain return; and Germany has not had long to wait for the ingathering of the fruits of her far-sighted policy. Thirty or forty years ago, industry could scarcely be said to exist there; it is now widely spread and successful."

Dr. Andrews concluded by saying that " Whatever be the result of our efforts to advance science and industry, it requires no gift of prophecy to declare that the boundless resources which the supreme author and upholder of the universe has provided for the use of man will, as time rolls on, be more and more fully applied to the improvement of the physical and, through the improvement of the physical, to the elevation of the moral condition of the human family. Unless, however, the history of the future of our race be wholly at variance with the history of the past, the progress of mankind will be marked by alternate periods of activity and repose; nor will it be the work of any one nation or of any one race. To the erection of the edifice of civilised life, as it now exists, all the higher races of the world have contributed; and if the balance were accurately struck, the claims of Asia for her portion of the work would be immense, and those of Northern Africa not insignificant. Steam power has of late years produced greater changes than probably ever occurred before in so short a time. But the resources of Nature are not confined to steam, nor to the combustion of coal. The steady water-wheel and the rapid turbine are more perfect machines than the stationary steam-engine; and glacier-fed rivers with natural reservoirs, if fully turned to account, would supply an unlimited and nearly constant source of power depending solely for its continuance upon solar heat. But no immediate dislocation of industry is to be feared, although the turbine is already at work on the Rhine and the Rhone. In the struggle to maintain their high position in science and its applications, the countrymen of Newton and Watt will have no ground for alarm so long as they hold fast to their old traditions, and remember that the greatest nations have fallen when they relaxed in those habits of intelligent and steady industry upon which all permanent success depends."

Two Evening Lectures were delivered; one by Prof. Tait, "On Force," and one by Sir Wyville Thomson, "On some Results of the *Challenger* Expedition." In his lecture "On Force" Prof. Tait observed that some criticisms on works in which he had at least had a share had shown him that even among the particularly well-educated class who wrote for the higher literary and scientific journals, there was widespread ignorance as to some of the most important elementary principles of physics. He had therefore chosen as the subject of a lecture a very elementary but much abused and misunderstood term, which met us at every turn in the study of natural philosophy. If one had a right to judge of the general standard of popular scientific knowledge from the statements made in the average newspaper, or even from those made in some of the most pretentious among so-called scientific lectures, there could be but few people in this country who had an accurate knowledge of the proper scieentific meaning of the little word "force." We read constantly of the so-called "Physical Forces"—heat, light, electricity, &c.; of the "Correlation of the Physical Forces;" of the "Persistence or Conservation of Force." To an accurate man of science all this was simply error and confusion, and he had full confidence that the inherent vitality of truth would render the attempt to force such confusion upon the non-scientific public quite as futile as the hopelessly ludicrous endeavour of the "Times" to make us spell the word 'Chemistry" with a "y" instead of an "e." There was no objection to such phrases as "the force of habit," "the force of example," &c.; but when they read, as he had, in one newspaper, that the "force" of a projectile from the 81-ton gun had at last reached the extraordinary amount of 1450 feet, in another that the "force" of a ball from the great Armstrong gun lately made for the Italian Government was expected to average somewhere about 30,000 foot-tons, and in a third that the water in the boiler of the *Thunderer* "would in a second of time generate force sufficient to raise 2000 tons 1 foot high," they saw that there must be somewhere at least, if not everywhere, a most reckless abuse of lauguage. In fact they had come to what ought to be scientific statements, and there even the slightest unnecessary vagueness was altogether intolerable. Perhaps no scientific English word had been so much abused as the word "force." We hear of "Accelerating Force," "Moving Force," "Centrifugal Force," "Living Force," "Projectile Force," "Centripetal Force," and what not. Yet there was but one idea denoted by the word, and all force was of one kind, whether it was due to gravity, magnetism, or electricity. This alone served to give a preliminary hint that there was probably no such thing as force at all, but that it was merely a convenient expression for a certain "rate." Much of the confusion about Force was due to Leibnitz and some of his associates and followers, who, whatever they may have been as mathematicians, were certainly grossly ignorant of some elementary parts of dynamics, insomuch that Leibnitz himself was known to have considered the fundamental system of the "Principia" to be erroneous, and to have devised another and different system of his own. This fact was carefully kept back now-a-days, but it was a fact, and it had a great deal to do with the vagueness of the terms for *Force* and *Energy* in some modern languages. In fact, in their modern dress, the *Vis Viva*, *Vis Mortua*, and *Vis Acceleratrix* of that time had, in some of their Protean shapes, hooked themselves, like Entozoa, into the great majority of our text-books.

The lecturer then proceeded to consider our modes of becoming acquainted with the physical world. In dealing with physical science it was absolutely necessary to keep well in view the all-important principle that—

"*Nothing can be learned as to the physical world save by observation and experiment, or by mathematical deductions from data so obtained.*"

The notion of force was suggested to us by the so-called muscular sense, which gives us a peculiar feeling of pressure when we attempt to move a piece of matter. The sense in which Newton used the word "force," and therefore the sense in which we must continue to use it if we desired to avoid intellectual confusion, would appear clearly from a brief consideration of his simple statement of the laws of motion. The first of these laws was—

"*Every body continues in its state of rest or of uniform motion in a straight line, except in so far as it is compelled by impressed forces to change that state.*"

In other words, any change, whether in the direction or in the rate of motion of a body, was attributed to force. Thus a stone let fall moved quicker and quicker, and we said that a force (viz., the weight of the stone, or the earth's attraction for it) was continually acting so as to increase the rate of the motion. If the stone were thrown upwards the rate of its motion continually diminished, and we said that the same force (the stone's weight) was continually acting so as to produce this diminution of speed. But this gave only half of the information which Newton's first law afforded. The moon revolved about the earth, and the earth and other planets revolved about the sun—approximately, at least, in circles. Why was this? Their directions of motion were constantly changing; in fact, a curved line was merely a line whose direction changed from point to point, while a straight line was one whose direction did not change; but to produce this change of direction force was required just as much as to produce change of speed. That was supplied by the gravitation attraction of the central body of the system. The old notion was that a centripetal force was required to balance the so-called centrifugal force, it being imagined that a body moving in a circle had a tendency to fly outwards from the centre! Newton's simple law exposed the absurdity of this. If a body was to be made to move in a curved line instead of its natural straight path, we must apply force to compel it to do so—certainly not to prevent it from flying outwards from the centre, about which it was for the moment revolving. In fact inertia meant not revolutionary activity, but dogged perseverance, and just as we must apply force in the direction of motion to change the rate of motion, so we must apply force perpendicular to the direction of motion to change that direction. Newton's second law was now required:—

"*Change of motion is proportional to the impressed force, and takes place in the direction of the straight line in which the force acts.*"

This one simple law held for all kinds of force alike. Change of motion was change of momentum, or the product of the mass of the moving body into its change of velocity. Of course the longer a given force acted the greater would be the change of momentum which it produced; so that to compare forces, which was the essence of the process of measuring them, we must give them equal times to act,—or, in scientific language, we must measure a force by the rate at which it produced change of momentum. Rate of change of velocity was called, in kinematics, acceleration. Thus the measure of a force was the product of the mass of the body moved into the acceleration which the force produced in it. This was the so-called *Vis motrix*, or "moving force" of the Cambridge text-books:—the so-called *Vis acceleratrix*, or "accelerating force," being really no force at all, but another name for the kinematical quantity acceleration which he had just defined. Thus unit force was that force which, whatever its source, produced unit momentum in unit of time. If we employed British units—unit of force was that which, in one second, gave to one pound of matter a velocity of one foot per second. A pound of matter was a certain mass or quantity of matter. The weight of a pound of matter varied from place to place on the earth's surface—it depended on the attracting as well as the attracted body. The mass of a body was its own property. The earth's attraction for a body, or the weight of the body, was a force which produced in it in one second, a velocity which (in this latitude, and at the sea-level) was about 32·2 feet per second. Some people were in the habit of confounding force with momentum, but no one having sound ideas of even elementary mathematics could be guilty of this or any similar monstrosity. But to show to a non-mathematician that it was really monstrous to confound force and momentum, it was sufficient to change the system of units employed in measuring them, when it would be found that, if numerically equal for any one system of units, they were necessarily rendered unequal by a mere change of the unit employed for time. Now two things which were really equal to one another must necessarily be expressed by the same numerical quantity whatever system of units was adopted. Unit momentum was that of one pound of matter moving with a velocity of one foot per second. Unit force was that force which, acting for one second, produced in unit of mass a velocity of one foot per second. In each of these statements we might put an ounce or a ton, instead of a pound, and an inch or a mile in place of a foot, and their relative value would not be altered. But if we took a minute instead of a second as the unit of time, one foot per second was 60 feet per minute—so this change of the time unit increased sixty-fold the nominal value of the momentum considered. But in the case of the force our statement would stand thus:—What we formerly called unit of force was that which, acting for one-sixtieth only of our new unit of time produces in a mass of one pound sixty-fold the new unit of velocity. In other words, the number expressing the momentum was increased sixty-fold, while that representing the force was increased three thousand six hundred fold. In fact, whatever system of units we employed—if we increased in any proportion the unit of time, the measure of a momentum was increased in that proportion simply, while that of a force was increased in the duplicate ratio. The two things were, therefore, of quite dissimilar nature, and could not lawfully be equated to one another under any circumstances whatever. The mathematician expressed this distinction at once by saying that momentum was the time-integral of force, because force was the rate of change of momentum. Prof. Tait proceeded to say that the meaning of Newton's two first laws left absolutely no doubt as to the only definite and correct meaning of the word force. It was obviously to be applied to any pull, push, pressure, tension, attraction, or repulsion, &c., whether applied by a stick or a string, a chain or a girder; or by means of an invisible medium such as that whose existence was made certain by the phenomena of light and radiant heat, and which had been shown with great probability to be capable of explaining the phenomena of electricity and magnetism. There was then no such thing as centrifugal force; and accelerating force was not a physical idea at all. But that which was denoted by the term living force, though it had absolutely no right to be called force, was something as real as matter itself. To understand its nature we must have recourse to Newton's third law of motion, which was to the effect that—

"*To every action there is always an equal and contrary reaction; or, the mutual actions of any two bodies are always equal and oppositely directed.*"

This law Newton first showed to hold for ordinary pressures, tensions, attractions, impacts, &c. And when he said—"If any one presses a stone with his finger his finger is pressed with an equal and opposite force by the stone," we begin to suspect that force was a mere name—a convenient abstraction—not an objective reality. If we pulled one end of a long rope, the other being fixed, we could produce a practically infinite amount of force, for there was stress across every section throughout the whole length of the rope. If we pressed upon a movable piston in the side of a vessel full of fluid we produced a practically infinite amount of force—for across every ideal section of the liquid a pressure per square inch was produced equal to that which we applied to the piston. If we let go the rope, or ceased to press on the piston, all this practically infinite amount of force was gone! But Newton proceeded to point out that this third law was true in another and much higher sense. He said:—

"*If the action of an agent be measured by the product of its force into its velocity; and if, similarly, the reaction of the resistance be measured by the velocities of its several parts into their forces, whether these arise from friction, cohesion, weight, or acceleration, action and reaction, in all combinations of machines, will be equal and opposite.*"

The actions and reactions which were here stated to be equal and opposite, were no longer simple forces, but the products of forces into their velocities; *i.e.*, they are what were now called rates of doing work; the time-rate of increase, or the increase per second of a very tangible and real something, for the measurement of which rate Watt introduced the practical unit of a horse-power, or the rate at which an agent worked when it lifted 33,000 pounds 1 foot high per minute against the earth's attraction. With a moderate exertion we can raise a hundredweight a few feet, and in its descent it might be employed to drive machinery, or to do some other species of work. But tug as we pleased at a ton, we could not lift it; and therefore, after all our exertion, it would not be capable of doing any work by descending again. Thus it appears that force was a mere name, and that the product of a force into the displacement of its point of application had an objective existence. In fact, modern science showed us that force was merely a convenient term employed for the present to shorten what would otherwise be cumbrous expressions; but it was not to be regarded as a thing, any more than the bank rate of interest, be it 2, 2½, or 3 per cent., is to be looked upon as a sum of money, or than the birth-rate of a country is to be looked upon as the actual group of children born in a year. In fact, a simple mathematical operation showed us that it was precisely the same thing to say:—

"*The horse-power or amount of work done by an agent in each second is the product of the force into the average velocity of the agent,*"

and to say—

"*Force is the rate at which an agent does work per unit of length.*"

Following a hint given by Young, we now employed the term energy to signify the power of doing work, in whatever that power might consist. The raised mass, then, possessed, in virtue of its elevation, an amount of energy precisely equal to the work spent in raising it. This dormant, or passive, form was called potential energy. Excellent instances of potential energy were supplied by water at a high level, in virtue of which it could in its descent drive machinery—by the wound-up "weights" of a clock, which in their descent kept it going for a week; by gunpowder, the chemical affinities of whose constitutes were called into play by a spark, &c., &c. Another example of it was suggested by the word "cohesion," employed in Newton's statement, and which must be taken to include what are called molecular forces in general, such as, for instance, those upon which the elasticity of a solid depends. When we drew a bow, we did work, because the force exerted had a velocity; but the drawn bow (like the raised weight) had in potential energy the equivalent of the work so spent. That could in turn be expended upon the arrow. Now Newton spoke of one of the forms of resistance as arising from "acceleration." In fact the arrow, by its inertia, resisted being set in motion; work had to be spent in propelling it, but the moving arrow had that work in store in virtue of its motion. It appeared from Newton's previous statements that the measure of the rate at which work was spent in producing acceleration was the product of the momentum into the acceleration in the direction of motion, and the energy produced was measured by half the product of the mass into the square of the vivacity produced in it. This active form was called kinetic energy, and it was the double of this to which the term *vis viva*, or living force, had been erroneously applied. As instances of ordinary kinetic energy, or of mixed kinetic and potential energies, we might take the following:—A current of water capable of driving an undershot wheel; winds, which also were used for driving machinery; the energy of water-waves or of sound waves; the radiant energy which comes to us from the sun, whether it affected our nerves of touch or of sight (and therefore be called radiant heat or light) or produced chemical decomposition, as of carbonic acid and water in the leaves of plants, or of silver salts in photography (and be therefore called actinism); the energy of motion of the particles of a gas, upon which its pressure depends, &c. When the motion is vibratory the energy is generally half potential, half kinetic. These explanations and definitions being premised, we could translate Newton's words into the language of modern science, as follows:—

"*Work done on any system of bodies* (in Newton's statement the parts of any machine) *has its equivalent in work done against friction, molecular forces, or gravity, if there be no acceleration; but if there be acceleration, part of the work is expended in overcoming the resistance to acceleration, and the additional kinetic energy developed is equivalent to the work so spent.*"

But we had just seen that when work was spent against molecular forces, as in drawing a bow or winding up a spring, it was stored up as potential energy. Also it was stored up in a similar form when done against gravity, as in raising a weight. Hence it appeared that, according to Newton, whenever work is spent it is stored up either as potential or as kinetic energy, except, possibly, in the case of work done against friction, about whose fate he gave us no information. Thus Newton expressly told us that, except, possibly, when there is friction, work is indestructible, it is changed from one form of energy to another, and so on, but never altered in quantity. To make this beautiful statement complete, all that was requisite was to know what became of work spent against friction. Here experiment was requisite. Newton, unfortunately, seemed to have forgotten that savage men had long since been in the habit of making it whenever they wished to procure fire. The patient rubbing of two dry sticks together, or the drilling of a soft piece of wood with the slightly blunted point of a hard piece, was known to all tribes of savages as a means of setting both pieces of wood on fire. Here, then, heat was undoubtedly produced, but it was produced by the expenditure of work. In fact work done against friction had its equivalent in the heat produced. This Newton failed to see, and thus his grand generalisation was left, though on one point only, incomplete. The converse transformation, that of heat into work, dated back to the time of Hero at least. But the knowledge that a certain process would produce a certain result did not necessarily imply even a notion of the "why;" and Hero as little imagined that in his æolipile heat was converted into work, as did savages that work could be converted into heat. But whenever any such conversion or transference took place there was necessarily motion: and the mere rate of conversion or transference of energy per unit length of that motion was in the present state of science conveniently called force. No confusion could arise from using such a word in such a sense. Rumford and Davy showed conclusively that the materiality of heat could not be maintained, and thus gave the means of completing Newton's statement which, still farther extended and generalised by Colding and Joule, was now known as the law of the conservation of energy. The conception of kinetic energy was a very simple one, when visible motion alone was involved. And from motion of visible masses to those motions of the particles of bodies whose energy we call heat, was by no means a difficult mental transition. Heat and kinetic energy in general were no more "modes of motion" than potential energy of every kind, including that of unfired gunpowder, was a "mode of rest!" In fact a "mode of motion" was, if the word motion be used in its ordinary sense, purely kinematical, not physical; and if motion were used in Newton's sense, it referred to momentum, not to energy. The conception of potential energy, however, was not by any means so easy or direct. In fact, the apparently direct testimony of our muscular sense to the existence of force made it at first much easier for us to conceive of force than of pote tial energy. Why two masses of matter possess potential energy when separated, in virtue of which they are conveniently said to attract one another, was still one of the most obscure problems in physics. If the ingenious idea of the ultramundane corpuscles, the outcome of the life-work of Le Sage, and the only even apparently hopeful attempt which has yet been made to explain the mechanism of gravitation was true, it would probably lead us to regard all kinds of energy as ultimately kinetic. A singular quasi-metaphysical argument might be raised on this point, of which he could give only the barest outline. The mutual convertibility of kinetic and potential energy showed that relations of equality, though not necessarily of identity, could exist between the two, and thus that their proper expressions involved the same fundamental units, and in the same way. Thus, as we had already seen, that kinetic energy involved the unit of mass and the square of the linear unit directly, together with the square of the time-unit inversely, the same must be the case with potential energy; and it seemed very singular that potential energy should thus essentially involve the unit of time if it did not ultimately depend in some way on energy of motion. In defence of accuracy, which was the *sine quâ non* of all science, we must be "zealous," as it were, even to "slaying." And, as all the power of the *Times* would not compel us to put a *y* instead of an *e* into the word chemist, so neither would the bad example of Germany and France, though recommended to us with all the authority which might be attributed to an ex-president of the Association, succeed in inducing us to attach two or more perfectly distinct and incompatible scientific meanings to that useful little word, "force," which Newton had once and for ever defined for us with his transcendent clearness of conception.

The usual Lecture to Working Men was delivered by Lieut. Cameron, R.N., C.B., on his "Recent Journey across Africa."

The Mathematical and Physical Section was presided over by Professor Sir William Thomson, F.R.S. His opening address was mainly devoted to a review of evidence regarding the Physical Condition of the Earth; its Internal Temperature; the Fluidity or Solidity of its Interior Substance; the Rigidity, Elasticity, Plasticity of its External Figure; and the Permanence or Variability of its Period and Axis of Rotation. He first, however, referred to his recent visit to America. In the United States Government part of the Great Exhibition of Philadelphia, Prof. Hilgard showed him the measuring rods of the United States Coast Survey with their beautiful mechanical appliances for end measurement, by which the three great baselines of Maine, Long Island, and Georgia were measured with about the same accuracy as the most accurate scientific measures whether of Europe or America have attained in comparing two metre or yard measures. In the United States telegraphic department he saw and heard Elisha Gray's splendidly worked-out electric telephone actually sounding four messages simultaneously on the Morse code, and clearly capable of doing yet four times as many with very moderate improvements of detail; he also saw Edison's automatic telegraph delivering 1,015 words in 57 seconds; this done by the long-neglected electro-chemical method of Bain, long ago condemned in England to the helot work of recording from a relay, and then turned adrift as needlessly delicate for that. In the Canadian department he heard "To be or not to be—there's the rub," through an electric wire; but, scorning monosyllables, the electric articulation rose to higher flights, and gave him passages taken at random from the New York newspapers with unmistakable distinctness by the thin circular disc armature of a small electro-magnet. The words were shouted with a clear and loud voice by Prof. Watson at the far end of the line, holding his mouth close to a stretched membrane, carrying a little piece of soft iron, which was thus made to perform in the neighbourhood of an electro-magnet in circuit with the line motions proportional to the sonorific motions of the air. This, the greatest by far of all the marvels of the electric telegraph, was due to a young countryman of our own, Mr. Graham Bell, of Edinburgh and Montreal, and Boston. Who could but admire the hardihood of invention which devised such very slight means to realise the mathematical conception that, "if electricity is to convey all the delicacies of quality which distinguish articulate speech, the strength of its current must vary continuously and as nearly as may be in simple proportion to the velocity of a particle of air engaged in constituting the sound?"

The Patent Museum of Washington, an institution of which the nation was justly proud, and the beneficent working of the United States patent laws, deserved notice. He was much struck with the prevalence of patented inventions in the Philadelphia Exhibition. He asked one inventor of a very good invention "why don't you patent it in England?" The answer was "The conditions in England are too onerous." We certainly were far behind America's wisdom in this respect. If Europe did not amend its patent laws (England in the opposite direction to that proposed in the bills before the last two sessions of Parliament) America would speedily become the nursery of useful inventions for the world. He might also mention "Old Prob's" weather warnings, which cost the nation 250,000 dollars a year; and though Democrats or Republicans playing the "economical ticket" might for half a session stop the appropriations for even the United States Coast Survey, no one would for a moment think of starving "Old Prob." The United States' Naval Observatory was full of the very highest science under the command of Admiral Davis. If to get on to precession and nutation, he had resolved to omit saying that he had there, in an instrument for measuring photographs of the Transit of Venus seen, for the first time in an astronomical instrument, a geometrical slide, the verdict on the disaster on board the *Thunderer*, published while writing his address, forbade him to keep any such resolution, and compelled him to put the question, "Is there in the British Navy, or in a British steamer, or in a British land boiler another safety-valve so constructed that by any possibility, at any temperature, or under any stress it can jam?" and to say that if there was it must be instantly corrected or removed.

Passing to the subject of his address, Sir William Thomson said that the evidence of a high internal temperature was too well known to need any quotation of particulars at present. Below the uppermost ten metres stratum of rock or soil sensibly affected by diurnal and annual variations of temperature, there was generally found a gradual increase of temperature downwards, approximating roughly, in ordinary localities, to an average rate of 1 deg. C. per thirty metres of descent, but much greater in the neighbourhood of active volcanoes, and certain other special localities of comparatively small area, where hot springs and perhaps also sulphurous vapours prove an intimate relationship to volcanic quality. It was worthy of remark in passing that so far as we know at present there were no localities of exceptionally small rate of augmentation of underground temperature, and none where temperature diminishes at any time through any considerable depth downwards below the stratum sensibly influenced by summer heat and winter cold. By a simple effort of geological calculus it had been estimated that 1 deg. per 30 metres gives 1000 deg. per 30 kilometres, and 3333 deg. per 100 kilometres. This arithmetical result was irrefragable, but what of the physical conclusion drawn from it with marvellous frequency and pertinacity that at depths of from 30 to 100 kilometres the temperatures are so high as to melt all substances composing the earth's upper crust? It had been remarked, indeed, that "if observation showed any diminution or augmentation of the rate of increase of underground temperature in great depths, it would not be right to reckon on the uniform rate of 1 deg. per 30 metres, or thereabouts, down to 30, or 60, or 100 kilometres. But observation has shown nothing of the kind, and, therefore, surely it is most consonant with inductive philosophy to admit no great deviation in any part of the earth's solid crust from the rate of increase proved by observation as far as the greatest depths to which we have reached." Now he had to remark upon this that the greatest depths to which we have reached in observations of underground temperature was scarcely one kilometre; and that if any falling off of the rate of augmentation of underground temperature was sensible at a depth of one kilometre, this would demonstrate that within the last 10,000 years the upper surface of the earth must have been at a higher temperature than that now found at the depth of one kilometre.

Such a result was, no doubt, to be found by observation in places which had been overflown by lava in the memory of man or a few years further back; but if it were found for the whole earth, it would limit the whole of geological history to within 10,000 years, or, at all events, would interpose an absolute barrier against the continuous descent of life on the earth from earlier periods than 100,000 years ago. Therefore, although search in particular localities for a diminution of the rate of augmentation of underground temperature in depths of less than a kilometre might be of intense interest, as helping us to fix the dates of extinct volcanic actions which had taken place within 10,000 years or so, we know enough from thoroughly sure geological evidence not to expect to find it, except in particular localities, and to feel quite sure that we should not find it under any considerable portion of the earth's surface. If we admit as possible any such discontinuity within 900,000 years, we might be prepared to find a sensible diminution of the rate at three kilometres' depth, but not at anything less than 30 kilometres if geologists validly claim as much as 90,000,000 of years for the length of the time with which their science is concerned. Now, this implied a temperature of 100 deg. C. at the depth of 30 kilometres, allowed something less than 2000 deg. for the temperature at 60 kilometres, and did not require much more than 4000 deg. C. at any depth, however great, but did require at the great depths a temperature of, at all events, not less than about 4000 deg. C. It would not take much 'hurrying-up' of the actions with which they are concerned to satisfy geologists with the more moderate estimate of 50,000,000 of years. This would imply, at least, about 3000 deg. C. for the limiting temperature at great depths. If the actual substance of the earth, whatever it might be, rocky or metallic, at depths of from 60 to 100 kilometres, under the pressure actually there experienced by it, could be solid at temperatures of from 3000 deg. to 4000 deg., then we might hold the former estimate (90,000,000) to be as probable as the latter (50,000,000), so far as evidence from underground temperature could guide us; if 4000 deg. would melt the earth's substance at a depth of 100 kilometres, we must reject the former estimate, though we might still admit the latter; if 3000 deg. would melt the substance at a depth of 60 kilometres, we should be compelled to conclude that 50,000,000 of years was an over estimate. Whatever might be its age, we might be quite sure the earth was solid in its interior; not absolutely throughout its whole volume, for there certainly were spaces in volcanic regions occupied by liquid lava; but whatever portion of the whole mass was liquid, whether the waters of the ocean or melted matter in the interior, these portious were small in comparison with the whole; and we must utterly reject any geological hypothesis which, whether for explaining underground heat or ancient upheavals and subsidences of the solid crust, or earthquakes, or existing volcanoes, assumed the solid earth to be a shell of 30 or 100, or 500, or 1000 kilometres thickness, resting on an interior liquid mass. This conclusion was first arrived at by Hopkins, who might, therefore, properly be called the discoverer of the earth's solidity. He was led to it by a consideration of the phenonema of precession and nutation, and gave it as shown to be highly probable, if not absolutely demonstrated, by his confessedly imperfect and tentative deduction, but a rigorous application of the perfect hydrodynamical equations leads still more decidedly to the same conclusion. Sir William Thomson asked those who possessed the "Transactions of the Royal Society" for 1862, and of Thomson and Tait's "Natural Philosophy," Vol. 1, to draw the pen through sections 23-31 of his paper on the "Rigidity of the Earth," in the former, and through everything in sections 847-849 of the latter, which referred to the effect on precession and nutation of an elastic yielding of the earth's surface, for he had convinced himself that those conclusions at which he had arrived by a non-mathematical short cut were grievously wrong.

A little consideration sufficed to show him that a very slight deviation of the inner surface of the shell from perfect sphericity would suffice, in virtue of the quasi-rigidity due to vortex motion, to hold back the shell from taking sensibly more precession than it would give to the liquid, and to cause the liquid (homogeneous or heterogeneous) and the shell to have sensibly the same precessional motion as if the whole constituted one rigid body. But although so much could be foreseen readily enough, he found it impossible to discover, without thorough mathematical investigation, what might be the characters and amounts of the deviations from a rigid body's motion which the several cases of precession and nutation contemplated would present. The investigation, limited to the case of a homogeneous liquid enclosed in an ellipsoidal shell, had brought out results which had greatly surprised him. When the interior ellipticity of the shell is just too small, or the periodic speed of the disturbance just too great to allow the motion of the whole to be sensibly that of a rigid body, the deviation first sensible renders the precessional or nutational motion of the shell smaller by a small difference than if the whole were rigid, instead of greater, as he expected. The amount of this difference bears the same proportion to the actual precession or nutation as the fraction measuring the periodic speed of the disturbance (in terms of the period of rotation as unity) bears to the fraction measuring the interior ellipticity of the shell; and it is remarkable that this result is independent of the thickness of the shell, assumed, however, to be small in proportion to the earth's radius. The conclusions to which Sir William's investigations led, he considered to be absolutely decisive against the geological hypothesis of a thin rigid shell full of liquid. But, he proceeded, interesting in a dynamical point of view as Hopkins's problem is, it could not afford a decisive argument against the earth's interior liquidity. It assumed the crust to be perfectly stiff and unyielding in its figure; but if it consisted of continuous steel and 500 kilometres thick, it would yield very nearly as much as if it were india-rubber, to the deforming influences of centrifugal force and of the sun's and moon's attractions. The state of the case was shortly this:—The hypothesis of a perfectly rigid crust containing liquid violates physics by assuming preternaturally rigid matter and dynamical astronomy in the solar semi-annual and lunar fortnightly nutations; but tidal theory has nothing to say against it. On the other hand the tide, decides against any crust flexible enough to perform the nutations correctly with a liquid interior, or as flexible as the crust must be unless of preternaturally rigid matter.

But thrice to slay the slain; suppose the earth this moment to be a thin crust of rock or metal resting on liquid matter. Its equilibrium would be unstable. The upheavals and subsidences would be strikingly analogous to those of a ship which had been rammed; one portion of crust up and another down, and then all down. Whatever might be the relative densities of rock solid and melted, at or about the temperature of liquefaction, it was, he thought, quite certain that cold solid rock was denser than hot melted rock; and no possible degree of rigidity in the crust could prevent it from breaking in pieces and sinking wholly below the liquid lava. Something like this probably went on for thousands of years after solidification commenced; surface portions of the melted material losing heat, freezing, sinking immediately, or growing to thickness of a few metres when the surface would be cool and the whole solid dense enough to sink. "This process must go on until the sunk portions of crust build up from the bottom a sufficiently close-ribbed skeleton or frame, to allow fresh incrustations to remain bridging across the now small areas of lava pools or lakes. In the honey-combed solid and liquid mass thus formed there must be a continual tendency for the liquid, in consequence of its less specific gravity, to work its way up, whether by masses of solid falling from the roofs of vesicles or tunnels, and causing earthquake shocks, or by the roof breaking quite through when very thin, so as to cause two such hollows to unite or the liquid of any of them to flow out freely over the outer surface of the earth; or by gradual subsidence of the solid owing to thermodynamic melting, which portions of it under intense stress must experience according to his brother's theory. The results which must follow from this tendency seem sufficiently great and various to account for all that we learn from geological investigation of earthquakes, of upheavals, and subsidences of solid, and of eruptions of melted rock."

Leaving altogether now the hypothesis of a hollow shell filled with liquid, we must still face the question, how much does the earth, solid throughout, except small cavities or vesicles filled with liquid, yield to the deforming (or tide generating) influences of sun and moon? This question could only be answered by observation. A single infinitely accurate spirit level or plummet far enough away from the sea to be not sensibly affected by the attraction of the rising and falling water, would enable us to find the answer. Closely connected with the question of the earth's rigidity, and of as great scientific interest and even greater practical moment, was the question—how nearly accurate is the earth as a timekeeper? and another of, at all events, equal scientific interest—how about the permanence of the earth's axis of rotation? Peters and Maxwell, about thirty-five and twenty-five years ago, separately raised the question, how much does the earth's axis of rotation deviate from being a principal axis of inertia? and pointed out that an answer to this question was to be obtained by looking for a variation in latitude of any or every place on the earth's surface in a period of 306 days. Peters gave a minute investigation of observations at Pulkova in the years of 1841-42, which seemed to indicate at that time a deviation amounting to equal to about 3-40 seconds of the axis of rotation from the principal axis. Maxwell from Greenwich found seeming indications of a very slight deviation—something less than half a second—but differing altogether in phase from that which the deviation indicated by Peters, if real and permanent, would have produced at Maxwell's later time. On his (Sir William Thomson) begging Prof. Newcomb to take up the subject, he undertook to analyse a series of observations suitable for the purpose, which had been made in the United States Naval Observatory, Washington. A few weeks later he received from him a letter referring him to a paper by Dr. Nysen, of Pulkova Observatory, in which a negative conclusion as to constancy of magnitude or direction in the deviation sought for is arrived at from several series of the Pulkova observations between the years 1842 and 1872, and containing a statement of his own conclusions, which were also negative. From the discordant character of these results we must not, however, infer that the deviations indicated by Peters, Maxwell, and Newcomb were unreal. On the contrary, any that fall within the limits of probable error of the observations ought properly to be regarded as real. There was in fact a *vera causa* in the temporary changes of sea level due to meteorological causes, chiefly winds, and to meltings of ice in the polar regions, and return evaporations, which seemed amply sufficient to account for irregular deviations of from one-half to 1-20th second of the earth's instantaneous axis from the axis of maximum inertia, or, as he ought rather to say, of the axis of maximum inertia from the instantaneous axis." Sir William concluded his address by considering the variations in the earth's rotational period.

The report of the Committee for Testing Experimentally the Exactness of Ohm's Law, drawn up by Prof. Clerk Maxwell, was read by Mr. Chrystal. The result of this investigation is described as follows:—If a conductor of iron, platinum, or German silver of one square centimetre in section has a resistance of one ohm for infinitely small currents, its resistance when acted on by an electromotive force of one volt (provided its temperature is kept the same) is not altered by so much as the millionth of a millionth part. The report concludes:—"It is seldom, if ever, that so searching a test has been applied to a law which was originally established by experiment, and which must still be considered a purely empirical law, as it has not hitherto been deduced from the fundamental principles of dynamics. But the mode in which it has borne this test not only warrants our entire reliance on its accuracy within the limits of ordinary experimental work, but encourages us to believe that the simplicity of an empirical law may sometimes be an argument for its exactness, even when we are not able to show that the law is a consequence of elementary dynamical principles."

One of the most important papers contributed to the section was that by Professor Stokes on "The Phenomena of Metallic Reflection." Professor Stokes explained that when Newton's rings were formed between a lens and a plate of metal, and were viewed by light polarised perpendicularly to the plane of incidence, it was known that, as the angle of incidence was increased, the rings which were at first dark-centred, disappeared in passing the polarising angle of the glass, and then reappeared white-centred, in which state they remained up to a growing incidence, when they could no longer be followed. At a high incidence the first dark ring was much the most conspicuous. To follow the rings beyond the limit of total internal reflection, a prism must be employed. When the rings formed between glass and glass were viewed in this way, as the angle of incidence was increased the rings one by one opened out, uniting with bands of the same respective orders which were seen beneath the limit of total internal reflection; the limit or boundary between total and partial reflection passed down beneath the point of contact, and the central dark spot was left isolated in a bright field. Now, when the rings were formed between a prism with a slightly convex base and a plate of silver, and the angle of incidence was increased so as to pass the critical angle, if common light be used in lieu of a simple spot, we had a ring which became more conspicuous at a certain angle of incidence well beyond the critical angle, after which it rapidly contracted and passed into a spot. To study the phenomenon in its purity it is necessary to employ polarised light, or, which is more convenient, analyse the reflected light by means of a prism. This phenomenon was discovered by Professor Stokes many years ago, but he has only just completed the necessary investigations.

Dr. Ker, to whose valuable discovery of the double refracting property produced in a dielectric by electric induction we referred in a recent number of this journal, described "An Experiment proving Rotation of the Plane of Polarisation of Light Reflected from a Magnetic Pole." If plane polarised light be allowed to fall on the polished extremity of the soft iron core of an electro-magnet, and a Nicol's prism be fixed in such a position as to extinguish the reflected light, the current being not yet sent through the coil of the magnet, on sending the current, the light is restored. In order to increase the magnetisation of the reflecting surface, or that portion of it which is utilised, the wedge-shaped termination of a mass of iron is held close over it. Sir W. Thomson remarked that this experiment proved iron to possess, in an enormously higher degree, the property discovered by Faraday for heavy glass.

In a paper "On the Protection of Buildings from Lightning," Professor Clerk Maxwell proposed to take advantage of the fact that an electric discharge cannot occur between two bodies unless their difference of potential was sufficiently great compared with the distances between them, but it was shown by experiment that if every part of the surface surrounding a certain region is at the same potential, every point within that region must be at the same potential, provided no charged body is within the region. If a powder mill were coated on the roof, walls, and floor with sheet copper, if all conductors, such as water-pipes, entering the building were connected with the coating, no electrical effect could be produced in the interior: but in practice, even this was superfluous in this climate. For ordinary buildings it would suffice to have a copper wire carried round the foundation of the house, up each of the corners and gables, and along the ridges. The copper wires might be built into the walls to prevent theft. In the case of a powder-mill, it might be advisable to make the network closer by carrying wires over the walls. It was advisable not to erect a tall condactor with a sharp point in order to relieve the thunder clouds of their charge.

A paper "On the Influence of the Residual Gas on the Movement of the Radiometer" was read by Mr. Crookes. His recent experiments show that the movement of this instrument is not due to a direct repulsion exerted by light on the vanes, but to a mutual action called out between these vanes and the very attenuated gas remaining in the instrument. It is well known that, with a moderately good vacuum, the motion becomes more rapid as the exhaustion proceeds; but Mr. Crookes has recently succeeded in producing such a complete exhaustion that he not only reaches the point of maximum effect, but goes far beyond it, so far that the effect nearly ceases. He measures the vacuum by means of a special apparatus, in which, instead of continuously rotating in one direction. as in the ordinary radiometer, the moving part is suspended by a glass fibre, which it twists in opposite directions alternately. The movement is started by rotating the whole apparatus through a small angle, and the observation consists in noting the successive amplitudes of vibration when the instrument is left to itself, a mirror and spot of light being employed for this purpose. The amplitudes form a decreasing series, with a regular logarithmic decrement. The logarithmic decrement is nearly constant up to the point at which the vacuum is apparently equal to a Torricellian vacuum, the mercury in the gauge standing at the same height as a barometric column beside it; but as the exhaustion proceeds beyond this point, the logarithmic decrement becomes smaller; in other words, the amplitude diminishes less rapidly. By plotting the observations and supposing the curve continued, it is indicated that, if a perfect vacuum were attained, the logarithmic decrement would be zero, we should have perpetual motion with constant amplitude, and at the same time, the radiometer would cease to act. He had tried other gases as well as air. Aqueous vapour is very unfavourable to the action of the radiometer; hydrogen, on the contrary, gave the best result of all. Prof. O. Reynolds and Mr. Schuster had published experiments which seemed to point to the true explanation of the action of the radiometer; but he thought Mr. Stoney's explanation the clearest. The molecules of the gas are beating about with the temperature; but, in tolerably dense air, they jostle one another, and their paths are short. When exhaustion is carried to a certain high point, the molecules are sufficiently few, and their paths sufficiently long for them to rebound from the glass.

We are compelled from want of space to omit notices of other valuable papers brought before Section A. These, with a report of the work done in the other sections, will be given in our next number.

MICROSCOPY.—Mr. A. M. Edwards, of Newark, New Jersey, has experimented upon the properties of salicylic acid as a preservative for microscopical purposes. Casts of uriniferous tubules, obtained from a severe case of nephritis, mounted in 1874 in a dilute solution, are in as good condition as when first put up. Mr. Edwards has also succeeded with *Volvox globatur*. Salicylic acid is worth further experiment, as our preservative fluids are as yet but imperfectly understood, and researches upon their properties much needed.

Labarraque's solution, so frequently mentioned in various American methods of staining vegetable tissues, and used for bleaching preparatory to the dyeing process, is the "*Liquor Soda Chlorinata*" of the British Pharmacopœia.

Mr. William A. Rogers contributes to the "Proceedings of the American Academy of Arts and Sciences" a paper "On a Possible Explanation of the Method Employed by Nobert in Ruling his Test Plates." Mr. Rogers has himself succeeded in ruling plates as fine as 80,000 lines to the inch. The paper contains three tables relating to the errors in spacing in plates ruled by Rutherford, Nobert, and the author, and a somewhat elaborate account of the mechanical contrivances employed in moving the plate to be ruled over given and equal spaces. The comprehension of this portion would be greatly aided by suitable illustrations. The tool used for ruling by Mr. Rutherford and Mr. Rogers is a diamond having a knife edge; that used by Nobert is known only to himself. The stone is worked artificially to the required form, the use of the natural crystal, either whole or broken into chance fragments, having long ago been abandoned. In these experiments it has been discovered that glass has what may for want of a better term be described as a *grain*, fine, clean lines being only capable of being ruled in certain directions. It is a remarkable fact that a properly-shaped diamond, set in the best position, is not at first capable of ruling the finest lines, but improves with use. When the diamond does its work perfectly the cut, even of the finest line, produces a sharp, singing sound, so that the operator can judge of the quality of the lines ruled almost as well by hearing as by sight. The best results have, however, been obtained with a black carbon worked to a knife edge: this stone is much harder than any other variety of diamond, the process of grinding to an edge occupying from five to ten days. Mr. Rogers's investigations have been the result of experiments undertaken for a different purpose. Failing to find a spider that would spin suitable web for the telescope of the meridian circle of Harvard College Observatory, an effort was made to produce upon glass lines of the desired quality and size. The object was accomplished, and the investigations have been continued during a period of about three years.

MINERALOGY.

Dr. F. A. Genth has recently published a paper "On Some American Vanadium Minerals." He first mentions Roscoelite, a vanadium mica found by Dr. Blake, of San Francisco, California, named and called by him "Roscoelite," in honour of Professor Roscoe, whose important investigations have put vanadium in its proper place among the elements. Roscoelite occurs in small seams, varying in thickness from 1-20th to 1-10th of an inch in a decomposed yellowish, brownish, or greenish rock. These seams are made up of small micaceous scales, sometimes ¼ of an inch in length, mostly smaller and frequently arranged in stellate or fan-shaped groups. They show an eminent basal cleavage; soft; the sp. gr. of the purest scales (showing less than 1 per cent. of impurities) was found to be 2·938; another specimen of less purity gave 2·921; lustre pearly, inclining to submetallic; colour, dark clove-brown to greenish brown, sometimes dark brownish green. Before the blowpipe it fuses easily to a black glass, colouring the flame slightly pink. With salt of phosphorus gives a skeleton of silicic acid, a dark yellow bead on the oxidising flame, and emerald green bead in the reducing flame. Only slightly acted upon by acids, even by boiling concentrated sulphuric acid; but readily decomposed by dilute sulphuric acid, when heated in a sealed tube at a temperature of about 180° C., leaving the silicic acid in the form of white pearly scales, and yielding a deep bluish green solution. With sodic carbonate it fuses to a white mass.

A remarkable deposit of tripolite, existing in the island of Barbadoes, where it is mixed with a certain quantity of carbonate of lime, has been examined by Dr. T. L. Phipson. Under the microscope it is found to be exceedingly rich in remains of fossil Infusoria, the forms of which are very well preserved. The silica is hydrated and soluble to a great extent in potash solution, and, like tripolite from other localities, it constitutes an excellent polishing material. On account of its value in this respect tripolite has many imitations in commerce, but it can be recognised at once by analysis, and also by the microscope. Among other useful purposes to which the Barbadoes tripolite has been applied latterly, we may mention that, having been found a bad conductor of heat, it has been used with advantage for covering boilers.

In a paper on "American Tellurium and Bismuth Minerals," read before the American Philosophical Society at the meeting of August 21, 1874, Dr. Genth mentioned, on the authority of Mr. P. Knabe, a siskin green pulverulent mineral from the "Iron Rod Mine," Silver Star District, Montana, as a *new* "Tellurate of lead and copper." At that time he had no opportunity of examining into the merits of this mineral. A qualitative examination proved it to be a hydrous *vanadate* of lead and copper and *not a tellurate*.

The nickel ores of New Caledonia are found by M. J. Garnier to be not arsenio-sulphides like those hitherto utilised, but silicates of nickel and magnesia. The ore is found amidst the masses of serpentine very abundant in certain parts of the island, and associated with euphotides, diorites, amphibolites, &c. The nickel is accompanied by iron, chrome, and cobalt; these metals, especially the two former, are of an unexampled abundance. The cobalt is associated with manganese. These ores have also been examined by MM. P. Christofle and H. Bouilhet, who find that they belong to three distinct types—An emerald green hydrosilicate, compact and hard, containing 18 to 20 per cent of nickel and 5 per cent of water; a yellowish green hydrosilicate, more friable, and containing 12 to 15 per cent of nickel and 10 to 15 of water; a whitish blue hydrosilicate, very brittle, and easily crushed with the fingers, containing merely 6 to 8 per cent of nickel, and as much as 20 per cent of water. The metallic nickel extracted from these ores contains from 98 to 99·5 per cent of pure nickel.

The Mineralogical Society of Great Britain and Ireland has issued the first number of its journal. A meeting of the society was held at Redruth on July 1st, when the secretary, Mr. J. H. Collins, read a paper on a new mineral from West Phœnix Mine which he has named "Henwoodite." It is a hydrous phosphate of alumina and copper, resembling turquoise, but containing more phosphoric acid and water and less alumina. Dr. C. Le Neve Foster read a paper on the occurrence of pyrophillite at Brookwood Mine, and on new mineral localities in Devon and Cornwall, in which he described a new mineral "Enysite," a hydrated compound of alumina and copper, with some sulphates, carbonates, and chlorides.

Mr. William Vivian contributed a note on paragenetic formations of carbonate of lime and oxide of iron, and of quartz, at the Mwyndy Mines, Glamorganshire. These specimens form interesting objects for the microscope, they are best seen as opaque objects in a good light and with a low power from 1" to 2" objectives. Mr. Collins remarked that these constant occurrences strengthen the evidence already existing that such iron ore deposits as that at Mwyndy, like those in the lodes or cross-courses of Cornwall and Devon, are due to infiltration of ferruginous matter from the surrounding rocks into pre-existing fissures.

At the same meeting Mr. Collins noted the occurrence of scorodite, pharmacosiderite, and olivenite in greenstones at Terras Mine, St. Stephens, and Dr. Foster exhibited a new form of blowpipe lamp suitable for travellers.

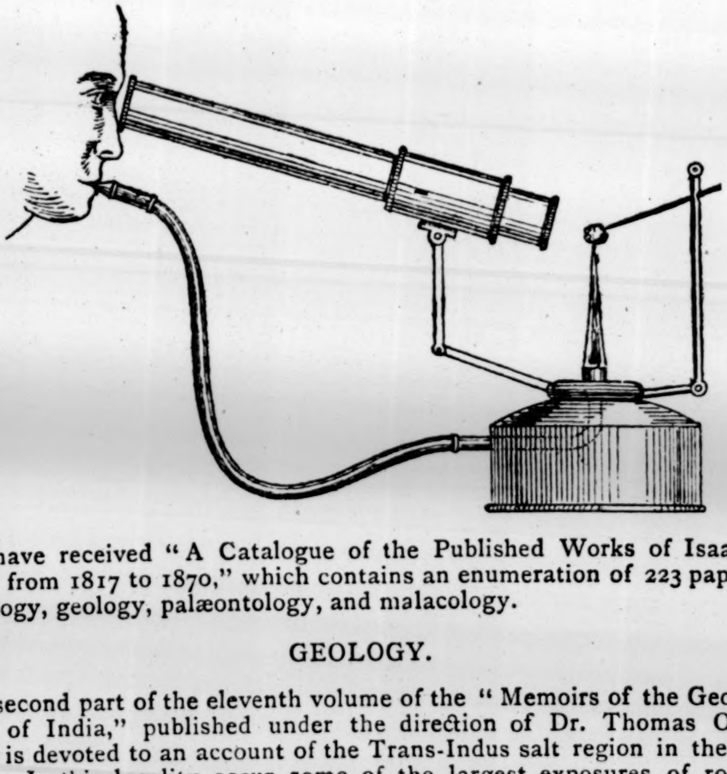
A meeting of the society was held in Glasgow on September 6th, when Professor Heddle delivered an address "On Scotch Minerals, how and where to find them."

Dr. Haughton, of Trinity College, Dublin, delivered an address on a new principle in the consolidation of porphyritic rocks. He received a collection of lava from Vesuvius, with the view of ascertaining if, in the space of 300 years, any difference had occurred in the composition of the lava flows. In the course of the investigation he ascertained that the minerals combined with a substance like paste, and his researches led to the conclusion that when a rock crystallises the maximum of minerals will form with a minimum of paste; this "principle of least paste" is confirmed by chemical experiments.

The Secretary read a paper by the president, Mr. H. C. Sorby, "On the Critical Point in the Consolidation of Granite." In the opinion of the author the critical point in the consolidation of granite is very closely connected with the critical temperature at which highly heated and compressed steam condenses into an equal volume of highly heated and expanded liquid water. Rocks melted under great pressure probably contain water either dissolved as a gas in a liquid or in the state of a fused hydrate. On cooling down to a lower temperature the crystallising out of minerals almost necessarily sets free the previously combined water. As long as the temperature is above the critical point it necessarily remains more or less disseminated in the rocks as a highly compressed steam, but as soon as the temperature falls below the critical point it condenses into highly expanded liquid water. The only determination of this temperature with which the author is acquainted is that of Cagmard Latur, who found that water expanded to nearly four times its volume, and passed into an equal volume of highly compressed steam, at about the melting point of zinc, or 412° C. As long as the temperature is higher than this the solvent action of water cannot be brought into play, since earthy substance and alkaline salts cannot be dissolved in steam; but as soon as it condenses into a liquid the solvent power of the water will be very great, as shown by its intense action on glass. These theoretical deductions agree remarkably well with the microscopical structure of the Pouza granite.

The Secretary read notes on a mineral from New South Wales, presumed to be lamontite, by Prof. Liversidge, F.G.S., "Some Notes from an Old Catalogue of Minerals," by Prof. A. H. Church, and "Notes on Occurrence of Achroite at the Rock Hill, in the parish of Austell, Cornwall, and on the Black Tourmaline of the same locality," by J. H. Collins.

The following is an illustration of an upright blowpipe with spectroscope adapted to its little lamp, contrived by Capt. Marshall Hall, for the purpose of discriminating in travelling between potass and soda and many other minerals. Capt. Hall remarks that "with hammer, chisel, lens, bottle of acid, magnetic penknife, and a little patience one can be far more independent of a laboratory than might be imagined. It is far more interesting to be able to determine a mineral on the spot, more especially as regards petrology, than to have to collect extensively and defer examination except with the blowpipe, which, as every one has to his aggravation experienced, leaves one sadly in the lurch when one gets amongst impure alkalies and alkaline earths. As an instance he has detected both baryta and strontia in arragonite, which blowpipe *solus* failed to show."



We have received "A Catalogue of the Published Works of Isaac Lee, LL.D., from 1817 to 1870," which contains an enumeration of 223 papers on mineralogy, geology, palæontology, and malacology.

GEOLOGY.

The second part of the eleventh volume of the "Memoirs of the Geological Survey of India," published under the direction of Dr. Thomas Oldham, F.R.S., is devoted to an account of the Trans-Indus salt region in the Kohat district. In this locality occur some of the largest exposures of rock salt known to exist upon the globe. The area occupied by these deposits is included within about a thousand square miles of country. The general aspect of the country is wild, rocky, and barren, almost bare of trees, and supporting nothing worthy of the name of jungle. The colouring of the ground is often vivid, whole ranges taking a green tint, not from grass but from the colour of the rocks, which are strongly contrasted with others of a bright purple. Zones of blood-red clays are varied by the white of adjacent gypsum, and interspersed with pale orange débris. The salt-beds occur in a nummulitic limestone, probably of the eocene period, and immediately below white, grey, and black gypsum with bands of dark grey clay, or black alum shale, sometimes impregnated with petroleum or bitumen. The upper part of the salt rock is bituminous, and its thickness varies from 300 to 1200 feet. In many places the salt is largely exposed, forming high detached hills and cliffs. It has a whitish grey colour, and varies in texture from a highly crystalline mass, its most common form to an earthy condition interspersed with finely divided clay. Much of the salt is remarkably pure, and so far as is yet known, it is without a trace of associated salts of potassa. The Kohat salt is found to contain—Insoluble matter, 0·5; sulphate of lime, 2·5; magnesia, calcium, and sulphates, 0·0. On the other hand, the best Cis-Indus salt contains nearly 2 per cent of foreign salts. The strata underlying the salt are as yet unknown. The difference between the salt deposits of the two regions are so unlike that even small samples, if not pulverised, are declared capable of being sworn to with confidence by the officials whose task it is to prevent the importation of the Kohat salt into the country east of the Indus. The rock salt of Persia, far to the westward, has probably no close connection with that of Kohat, since this overlies the nummulitic formation. The salt of Ormuz in its occurrence amongst dolerites, trachytes, and micaceous iron presents also no analogy with that of the Kohat. No organic remains have been discovered in the overlying gypsum, nor in the accompanying clays, except some obscure grass-like fragments. Some small impressions of shells are detected in a limestone band near the top of the gypseous formation. The resources of the district, in an economic point of view, are in addition to salt, gypsum, sulphur, alum, building stones, and coal. The latter mineral was first brought under notice by a native officer, and information concerning its quantity is as yet wanting. The total quantity of salt is estimated at 40 milliards of maunds, sufficient, after making am ample allowance for waste, to last, at the present rate of consumption, for 40,000 years.

In the eighth volume of the "Records of the Geological Survey of India," we find an interesting paper, by Mr. Fedden, on the former existence of ground ice in India. At Irai, in latitude 19° 53′, and at an elevation under 900 feet above the sea-level, the evidences of glacial action are considered as conclusive as those for the ice-age formation in Europe. There is also a notice of fire-bricks made from clay obtained at Mallapur. They were pronounced superior to Stourbridge bricks, but inferior to those of Glenboig. The Shapur coal-field, and the coal explorations in the Narbada region are described by Mr. H. B. Medlicott. The author thinks that there are good prospects in the Shapur district on the Tawa, if not in the Senada area. On the lower Narbada the hope of coal is somewhat more precarious. A sample from Naobelaka gave on analysis—Moisture, 3·4; volatile matter (not water), 39·6; fixed carbon, 55·2; ash, 1·8. Total, 100·0. Mr. Ball reports on the Raigarh and Hingir coal-fields. There are many seams of coal, that of Dibdorah being at least six and a half feet in thickness and of fair quality. Within the Barakar group there are two if not three zones of iron-stone, some of the ores appearing to be of good quality.

Volume nine, part 1, comprises the annual report of the Geological Survey of India for the past year. We may particularly notice Mr. Medlicott's labours in the coal-fields of the Satpure hills, where deep borings are recommended to test the bulk of the coal deposits and their depth beneath the surface. Dr. W. Waagen has been obliged to relinquish his connection with the survey on account of his health. The remainder of this issue is devoted to the geology of Scinde, 9000 square miles of which province have been subjected to a preliminary survey. The importance of a thorough examination of Scinde is due to two circumstances: it has long been known that there exists in that province a fine series of tertiary rocks, rich in fossils. Further, the figures and descriptions of the Indian nummulitic fossils, published by D'Archiac and Haime in their "Description des Animaux fossiles du Groupe Nummulitique de l'Inde," lose much of their value, from the circumstance that the exact position in the series of the beds from which the different fossils described were obtained, is unknown. The majority of the fossils have been procured in Scinde, but the exact localities were not recorded. The investigation of this province is in the hands of Mr. W. T. Blanford, F.R.S., the deputy superintendent of the survey.

The "American Journal of Science and Arts" contains a paper by Mr. G. K. Gilbert on "The Colorado Plateau Region considered as a Field for Geological Study." This region is 170,000 square miles in extent, and is remarkable for the rocky character of its surface and its extreme aridity. The only mineral product of economic importance is coal, which is worked only where the Pacific Railroad affords means of transport. "The air is so dry that, except on the heights and on the margins of springs and streams, there is no turf, no accumulation of humus, often no soil, and so little vegetation that the view is not obstructed. From a commanding eminence one may see spread before him, like a chart, to be read almost without effort, the structure of many miles of country. There is no need to search for exposure where everything is exposed. The strata are undisturbed, and are cut by valleys of erosion, in the wall-like sides of which every inch of the series may be examined." It is evident that such a region must afford remarkable facilities for studying mountain-building by displacement, stratigraphy, and the problem of the canons. The author remarks—"Already the field has yielded to its students results new to them, and probably new to the world of science. Among them are a type of uplifted mountains, a type of eruptive mountains, a theory of water-falls, and a classification of drainage-systems."

Under the title of "The Geology of Portions of our Western Territory," Mr. Gilbert gives the results of an examination of portions of Nevada, Utah, California, and Arigona. He treats of the orology of the district, of its valleys and canons, of the glacial epoch, the water-supply, the volcanic rocks and mountains, and of the stratified rocks. Concerning the glaciation, he infers that the general glaciation of the Eastern States had no counterpart, in the same latitudes over the region extending from the Rocky Mountains to the Sierra Nevada inclusive. There were in that region local glaciers high upon the flanks of the mountains, the most southerly of which did not extend lower than an altitude of 8000 feet above the sea. There was a general accession of water to the valleys of the great basin. Lakes were formed where there are now only deserts, and valleys now nearly empty were filled to overflowing. The flooding of the valleys is correlated in time with the formation of glaciers upon the mountain summits on the same principle on which the different local floods are correlated with each other, the local glaciers with each other, the glaciers of the East with those of the West, and those of America with those of Europe,—namely, that the phenomena were of the same class and occurred in the same division of geological time. Each took place during the post-tertiary, and each marked a climatal change of polar tendencies. The phenomena of the Glacial epoch at the West differed from the synchronous phenomena in the same latitudes at the East, for the reason that then, as now, the former region was comparatively arid, and material was lacking for a great ice-field. The configuration of continents was not so far different from the present but that the principal climatal districts were marked out, and the great flexures of the lines limiting zones of climates were arranged very much as we now know them." On the subject of water-supply the author enters upon a practical question of great moment, and especially interesting to an empire which embraces regions so deficient in rain as are certain parts of Australia and South Africa. What is the influence of agriculture upon climate? Will a cultivated country when brought under cultivation induce a larger amount of atmospheric precipitation than it did when in the state of a *desert*,—not, we must remember, of *forest?* Mr. Gilbert admits that the water of the Great Salt Lake has risen ever since, or nearly ever since, the occupation and cultivation of the neighbouring country by the Mormon settlers. He admits, further, that the rise of the lake indicates an increase of rainfall as compared with evaporation, and that large areas of land brought under cultivation are cooler than desert-surfaces, and hence better able to induce rainfall from currents of moist air. But he maintains that this increased rainfall is due to the increase of evaporation brought about by the wide distribution of water in irrigation, and that there seems no reason to believe that the result in precipitation shall exceed—if indeed it can equal—the expenditure in evaporation. This remark on the influence of irrigation—a practice very extensively resorted to in the Salt Lake district—has a significance which should not be overlooked. If it, as can scarcely be denied, reduces the temperature of the soil and increases the amount of rainfall, it must certainly have a generally pernicious effect in a country like England, where the temperature of the soil in average seasons might be advantageously increased, and where the rainfall is already so sufficient, in proportion to the evaporation, that any increase would ensure us a succession of wet summers and ruined harvests. It is, however, we think, demonstrated—even to superfluity—that the destruction of the forests has already reduced many regions in Europe, Asia, and Africa, to comparative sterility, and is evidently exercising an injurious effect in many others, as at the Cape. We have ourselves observed drizzling rain falling in a wood, whilst none could be perceived in the open country on all sides. Now if the destruction of vegetation can effect a decrease of the volume of water in the rivers, and, if not, reduce the total amount of rainfall during long periods of time, can still convert it from a regular and useful supply to an alternation of violent storms and long periods of drought, it does seem highly probable that planting should have an inverse result.

The ninth volume of Mr. F. V. Hayden's "Report of the United States Geological Survey of the Territories" is exclusively devoted to the invertebrate palæontology of the cretaceous and tertiary formation of the Upper Missouri country, and represents the result of a wonderful amount of labour and research. This will be at once understood when we say that 130 genera of fossil Mollusca have been examined and characterised, and a typical species of each described. The synonyms of the genera and species have been collated, and as far as possible determined. The illustrations consist of forty-five quarto plates, some of them containing more than thirty figures each and admirably illustrated. The whole work must be pronounced in the highest degree creditable to the geological staff of the United States Government, and as a most valuable contribution to palæontological science.

GLASGOW PHILOSOPHICAL SOCIETY

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London: Printed by E. J. Davey, Boy Court, Ludgate Hill, E.C.